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THE PSYCHOLOGICAL REVIEW

GESTALT PSYCHOLOGY IN RELATION TO OTHER PSYCHOLOGICAL SYSTEMS

BY W. LINE
University of Toronto

Traditional methods of discussing any supposedly new departure in science, almost invariably begin by showing how all the novel aspects of that departure are to be found in Hellenic thought. This is usually a comparatively simple task, since human thought at all times is cast into similar moulds; and since it is easy to read into concepts used by others, an interpretation which was foreign to, or only very implicit in, the former use. But despite the dangers of such a procedure, it is always necessary to consider the historical antecedents of any movement, if we are to view it in its true perspective. While accepting, therefore, the partial validity of the Gestalt protest against identifying their system with earlier tendencies,—since their methodological approach is somewhat unique,—we must none the less recall the main trends of psychological thought during the seventeenth, eighteenth and nineteenth centuries, as a basis for achieving a true understanding of present systematic positions.

Widespread familiarity with those trends, however, makes it unnecessary for us to dwell long on this aspect. The significance of Locke's empiricism and Hume's atomism in suggesting an approach to scientific psychology that would reveal the genesis of mental content; the taking over of this legacy by British Associationism and developing it one-sidely, assisted by the rapid advances in physiological psychology on the Continent; the influence of Biologism in re-interpreting the atomistic conception, are developments needing no elab-

oration here. Equally well-known are the Kantian protest; the implied dissatisfaction with mechanistic associationism in the view of Herbart, in Brown's revival of Locke's 'reflection,'—which Hartley had discarded,—and the consequent emphasis of 'relational elements'; the similar dissatisfaction with artificial atomism mirrored in Bain's law of relativity; and the continuous re-discovery of relations in various guises in German psychology of last century, leading to the present-day Gestalt emphasis, and to Spearman's doctrine of Noegenesis. Further inadequacies of Associationism are sensed in Mill's conception of 'Mental Chemistry,' in Wundt's 'Creative Synthesis' and even in Helmholtz' theory of unconscious judgments; while the post-evolutionary conception of mental growth, emphatically maintained and developed by Ward, and later by Stout, leads to a treatment of 'dispositions' and 'relations' in which many similarities to the Gestalt notion of 'patterns' are explicitly formulated.

The foregoing is by way of showing that the Gestaltist revolt against Associationism is not an isolated movement, or, in its gross aspects, decidedly novel. Its more immediate antecedents, however, are found in German and Austrian thought of last century. Growing out of the Kantian emphasis on the synthetic activity of mind, especially as regards its cognitive aspects, the Act Psychology of Brentano stressed, as the special field of psychology, the processes underlying both the appearance and sequences of mental content. In so doing, the important considerations become those of form, of unity—in the sense implied by the Platonic distinction. Formal aspects become the object of study, and whole qualities are explicitly recognised. Perhaps the earliest psychological interest in whole qualities as such, may be attributed to Mach and Von Ehrenfels, and to the Graz School under Meinong. But here the formal qualities were considered in terms of their genesis out of sensory data, the act of production being recognised, but shrouded in mystery under such rubrics as 'Attention,' 'Intellect,' and so forth. Formalism was attributed to mental activity of a higher order than that of sensory apprehension, but was descriptively dealt with in

terms of the characteristics of resultant phenomena. In this way there seems to have been added to Wundt's suggestion of additional, unexplained emergent qualities in mental synthesis—rather an after-thought in his system—nothing more than an elaboration of the nature of those form emergents—now designated *Gestalten*, shapes, shape moments, shape qualities, and the like.

The Berlin exponents of the doctrine of Form mention these antecedents only with a view to dissociating themselves therefrom. They swear by the insight of Wertheimer, who, in 1912, published a striking article[1] wherein the Formal organization of mental presentations was postulated as inherent in all phenomena. No act of production, no association of elements, was necessary; in fact, such speculation regarding the antecedents of organized mental life was misleading. If observation is turned towards such antecedents, we pass beyond the realm of direct experience, and commit the errors inherent in introspectionism; and one of the difficulties with introspectionism is that it unearths aspects or elements of experience which seem to be ultimate bases of mental phenomena, yet which are arrived at by a structural analysis of the immediately given, and from which the original organization can be derived only by invoking the aid of some *deus ex machina*. 'Meaning,' 'past experience,' 'association,' 'reproduction,' 'intellect' and the like are called in wherever needed, to explain the genesis of something that was, in point of fact, prior to the elements whose synthesis has now to be depicted if we are to arrive at our starting point and, at the same time, the original form has been destroyed by such analysis, beyond all hope of synthetic reconstruction; the whole is greater than the sum of its parts. Hence, for them, the truth lies in accepting the immediacy of the given configuration, and thence proceeding to discover the laws governing structured phenomena.

In this we appear to be involved in an essential difference of methodological approach to psychological problems. It is

[1] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 1912, 61, 161–278.

of course obvious that the difference is certainly one of starting point; but whether the methodology is essentially different is another matter. Difference in descriptive unit there certainly is; but that even here the difference is absolute and intrinsic, remains to be proved; and for the present it appears that the Gestaltists are so much concerned with converting heretics to the acceptance of their units, and instructing them as to the meaning of their terms, that the further steps are sadly lacking.

Their insistence on the importance of recognising the primary fact of sensory organization, suggests at once that we have here a re-emphasis on the relational aspects of experience. This has been a frequent tendency in psychology during the last century. Stout is a case in point; and, indeed, his *Analytic Psychology* is replete with passages that might easily have been written during the last ten years,—although his later *Manual*, even in the revised edition, savours more of antiquity. But before considering some of the details of the Gestalt doctrine, we may best proceed by completing our brief sketch of its development.

We have noted that the Berliners trace the origin of their movement to Wertheimer's 1912 pronouncement,—all previous psychology being characterized as a barren desert of associationism. Incidentally, Watson chooses the same year for the birthday of Behaviourism, before which, he claims, the conventional psychology of the soul held entire sway. We may pass over this plausible argument for Protagorean scepticism, and, at the same time, the ready explanation of so apparent a conflict between the two ways of reacting to psychological tradition, since the point is of only secondary importance here. Of more immediate interest, however, is the fact that Wertheimer's work seems to sink into insignificance when compared with that of his followers, Koffka and Köhler; for, apart from the experimental demonstration of the perception of movement as such, now made famous as the *Phi-Phenomenon*, he appears to have contributed little save a great deal of generalization from his somewhat limited results, including some rather intriguing suggestions as to the

spatial conditions under which wholes manifest themselves in the sensory (visual) field. Koffka's article of 1922 [2] really constitutes the first main statement concerning the Gestalt view of perception,—published shortly after Spearman had offered his doctrine of Noegenesis,—and since in later publications Koffka increasingly stressed the genetic method of study, he became involved in developing the picture of successive stages of structured phenomena. Hence, in endeavouring to depict the ever increasing complexity of mental configurations, from the earliest days of infancy onwards, he has to deal very specifically with the problems of learning. In this process, Thorndike's neural-associationism is singled out for special attack, and the maturation theory is related to the problem of achievement. To this development Köhler's experiments with apes made some contribution, and set up the god of 'insight'; and his subsequent speculations in the world of physics have furnished us with analogies, which appear to pay homage to the psychological custom of accepting physical speculation as explanatory of mental functions. Apart from this his main contribution lies in broad generalizations, born of enthusiasm, until the Gestalt notions furnish the key to all science, and indeed, to the entire cosmos. In so doing, however, Köhler has made still more explicit the application of configurational concepts to behaviour situations. Not merely the non-temporal, cross-sectional aspect of phenomenal organization, but also the temporal aspect, and the behaviour pattern are dealt with. Hence his stress on the dynamics of behaviour, activity in long-section, the goal and, by enlisting Lewin's experimental support, the concept of the Self is re-established in this dynamical setting, and Personalism seems to be raising its head once more unexpectedly.

So much for the main trends of the development. Let us turn to more specific illustrations in the fields of perception and learning. Here, their experimentation has been confined to certain fairly limited aspects of these fields, from which their system of psychology has sprung. It is our purpose to

[2] K. Koffka, Perception, an introduction to Gestalttheorie, *Psychol. Bull.*, 1922, 19, 531–585.

try to show the trend of their thought, based on typical experiments of their own, and to show how in both fields a coordinated emphasis prevails. In so doing, however, we may be able to isolate crucial points wherein apparent controversies may be resolved by particular lines of research.

In the field of Perception, Gestalt experiments have been very largely confined to vision. It is true that their illustrations (and some few experiments) have been dependent upon other sense modalities, especially audition; for the classic examples stress the form-quality of musical melody, of clangs, and so forth. These forms are not explicable in terms of their component parts; they are transposable,—not affected by change of the particular parts, as is involved in change of key, for instance. They are immediately given in perception, and are the necessary prior determinants of the interpretation of parts. Determinism runs from whole to parts, and not *vice versa*. Hence associationistic explanations are not valid. Thus runs the argument. What experimental evidence can be offered in support of these views?

Considerable importance is attached to experiments and descriptions concerning primitive experiences. Koffka has familiarised us with the conception of unified experience from the earliest days of life in terms of an indifferent, ill defined, hence uniform background, from which a limited and somewhat definite quality emerges.[3] Passing over, for the moment, the apparent inconsistency of including in such primitive configurations aspects of the phenomenal field which are, by definition or inference, uncognised, we next become involved in experimentation designed to prove that this view,—the view of experience as ordered from the first, by virtue of its being experience—is more acceptable than the conclusion that order results from association of manifold, discrete sensations (as if this were the only alternative). The paucity of infantile experiences, the apparent complexity of earliest stimuli (a friendly or angry face, for example), the argument by analogy with the phenomena of adults in repose or extreme passivity, the selective training experiments with animals and infants

[3] K. Koffka, Growth of the Mind, 1925.

whereby the response to relative qualities can be demonstrated, and so forth, all point to the significance of 'order' in primitive phenomena. Hence the dispute as to the priority of 'absolute' or 'relative' qualities takes the stage again.

In connection with this presentation of primitive phenomena, a certain degree of equivocality mars the scientific utility of the term 'whole.' It is sometimes applied (by Koffka, for instance) to an uncognised experience, equivalent to the 'big buzzing booming confusion' of James, from which we seem to expect the view that primary experience is a precognitive stage—the *Pathema* of Plato. It is similarly applied to other concepts uncognised by the subject, yet cognised by the experimenter; as in any of the behaviour configurations manifesting closure. It also covers the case of totalities uncognised by anyone, save in dire abstraction, such as the continuity of the ego, or even the cosmos. And again, most commonly, the cognitive configurations of all perception.

The argument for primitive, immediate wholes, is apparently based on the same type of instance as that presented by Bain in support of his law of relativity, where 'red' would supposedly not be known unless we knew at the same time 'not red.' Hence, the emergence, in experience, of a quality on a ground, demands the postulation of organized unity as an essential factor in cognition. From that, the priority of relative qualities is easily deduced, since it is the initial assumption—backed up, it is true, by some apparent experimental evidence. But a great deal of research has been done since the days of Köhler's original selective training experiments, with results which, while not necessarily conflicting, can be interpreted in several ways. For example, the interesting fact has been discovered that with progressive separation of the brightness-objects, the proportion of absolute choices increases; as, too, with progressive differentiation between the brightness values involved.

Whether or not the conflicting interpretations of such evidence can be resolved, matters little for present purposes. It may appear a detail to which we have already given undue

weight. But our purpose in so doing is to indicate, first, that a system based on evidence of this nature—and no one can deny the importance attached to like experiments by the Gestaltists—demands some further enquiry even at this level, before proceeding. Secondly, it appears obvious that associationistic atomism is not the only alternative to configurationism; for there is nothing so far in the argument, against the view of primitive experience as a pre-cognitive mental state, from which the apprehension of a quality follows immediately, and that cognitive priority is awarded to 'absolute' qualities. Mental unity prevails thus far, if we are particularly anxious to preserve it, and need never be vitiated if we add the introspectively observable (and universally accepted) process of relating. We shall see in a moment, however, why the Gestaltists profess disinterest in such an alternative.

Proceeding to experiments wherein the reaction to visual patterning is further demonstrated—as, for instance, in the work of Hertz—we are confronted with another difficulty. Animals undoubtedly do react as if sensory organization were the main determinant. The total constellation seems to be of prime significance, and the degree to which the 'right' object (i.e. the object to be selected) stands out from the rest of the field, determines the ease or difficulty of making the required adjustment. Just what does this 'standing out' process involve? It is difficult to say in terms of animal behaviour. But considering the problem from the human standpoint, there is unearthed the fact that 'standing out' means one of two things—apparent or relative dis-unity of the phenomenal field, or else more explicit relationship between the 'right' object and other aspects of the field. (Perhaps these are identical.) In other words, we have differing degrees of organization or unity. Continuous wholes seem to be separated out—a fact of common experience—organization within the whole is more intimate, relations between wholes are of a less permeating order.

We are here confronted with the crux of the problem of perception. If we adopt the Gestalt point of view, we must accept organization as an original sensory fact. The basis

of that organization must be examined only in terms of the laws inherent in organization as such—not in terms of process. Process analysis, in this case, does not interest them, except speculatively. We must not speculate, however, introspectively, or in terms of mental process, for this would savour too much of traditional days. Strangely enough, we may speculate in terms of cerebral processes; and in so doing, we discard the constancy hypothesis in its old form, and arrive at inter-connected systems of nervous excitation, analogous to physical distributions of potential in a network of electrified wires. This brings us back from another angle, to the laws inherent in the organization—supposedly without any metaphysical postulate. Some people none the less prefer Aristotle's doctrine of the Common Sense.

Let us examine this matter of varying degrees of intimacy further, since it is fundamental to their whole position. Experience is unified, but certain sub-wholes stand out originally. Therefore there must be laws of organization producing these hierarchies of wholeness. Wertheimer has attempted to isolate these laws, and has offered his principles of nearness, similarity, continuity, inclusiveness, and so forth. Such organization will become as perfect as the prevailing conditions admit, we are told; and this is the famous law of configuration, though just what meaning is to be attached to the term 'perfect' is difficult to say. The 'conditions,' however, are partly covered by the principles stated above; and these savour of the old principles of contiguity and similarity, with additions and new interpretations.

It appears, therefore, that in so far as the degree of organization of different parts of the sensory field varies, certain segments become relatively segregated. If adjustment is to be directed in terms of a segregated unit, it is easier than if a particular part of an intimate whole is involved. The adjustment is, of course, intended towards the part in question (the flower-pot-with-food-underneath, in Hertz' experiments); but if we place this particular pot in a spatial organization such that phenomenologically it doesn't exist (save as a part within a whole, *i.e.* as a new phenomenon) the adjust-

ment fails. If we preserve its identity to a requisite degree, the animal knows it still; and with animals of different levels of ability, the degree to which this relative dis-unity can be preserved varies. The breaking up of a Gestalt, or the preservation of a relatively dis-united field seem to depend on the prowess or nature of the organism. Consequently this latter must be added to Wertheimer's principles as a further condition determining the Gestalt.

When they say, therefore, that 'natural unities exist,' they must imply that they exist for some perceiving organism. The structure of those unities involves relations, complexity of structure being identical with complexity of relations. "The shape, as object, is impossible," says Benussi, "without the relations; but the presentation of the shape is not bound to the thought of the relations." Hence their position is that although objective shape remains constant, perceived shape varies due to some differences in the perceptual response.

But so far they have made no attempt to examine these differences as a basis for determining just what the underlying psychological processes are. They cannot do so, since their basic postulate suggests the primacy of the form. Hence again, as a substitute for psychological hypothesis, they interject neural organization, activated by potential differences.

All this results in a definite limitation in method. We may infer the degree of neural organization, with its concomitant phenomenal pattern, by the nature of the animal's responses. With human beings we supplement this by inner observation. But the laws governing the transition from one pattern to another in an objectively constant field, are necessarily hidden until neural physics enlightens the psychologist. Psychological process is neglected, save in the rather loose and one-sided conception of 'adopting an analytical attitude' whereby we may break up the Gestalt, if we are sufficiently advanced mentally so to do. Genetically we may, by their method, be able to map out the sequences of phenomena that are possible. Psychologically we are handicapped. Physically our studies may be scientific. Psychologically we are mere fact gatherers. It is fundamentally because of this fact

that the British reaction against Associationism has recently gone beyond that of Ward and Stout, and has tried to depict these same phenomena, discussed by the Gestaltists, in psychological terms. Spearman's hypotheses of Noegenesis are best appreciated in this setting; and by developing his techniques in the field of Individual Differences, he has been able to substantiate these hypotheses to some considerable degree.[4]

One further important characteristic of the Gestalt view of perception should be mentioned—namely, their peculiar antipathy to invoking the aid of past experience in explaining sensory organization. The 'meaning' theory is repeatedly decried as mystical. Objects do not stand out by virtue of meanings acquired in past sensory experience, and subsequently reproduced associatively. The organization takes care of that—the objective, natural wholes and their responding neural, similarly organized counterparts. Yet once a particular whole has been perceived, it seems to leave some sort of Stoutian Cognitive Disposition (it cannot be physiological entirely, with the constancy hypothesis gone!), which gives it an advantage in later experience. Support for this comes from Rubin, with his quaint reversible figures; and his experiments are amplified by the Berliners to show how powerful these dispositions can be. If a figure-ground combination has been seen one way, its memory value is easily demonstrated. Are we here presented with an inconsistency, owing to the fact that once the outcry against past experience has been used to attack associationism, it is forgotten somewhat as the alternative psychological system is developed?

The point is worthy of further consideration. Past experience must, of course, be given a place; for it most certainly differentiates two such phenomena as the following. If we have nine dots arranged spatially so that there are three horizontal rows of three dots, each row being similar to the others, and corresponding dots being placed in the same vertical line, we have several organizations possible, and need not invoke the aid of past experience in describing their genesis (their appearance). But if we have a pictorial figure, such

[4] C. Spearman, The Nature of Intelligence, 1923, and The Abilities of Man, 1927.

as Jastrow's Duck-Rabbit, or Spearman's Bride-Mother-in-law,[5] many secondary factors are necessarily involved; and without this distinction between primary and secondary perception, how else are we to arrive at the nature of the psychological processes? If we say that past experience is not necessary in order that two figure-ground phenomena may occur in these pictorial examples, we must acknowledge a possible and frequent effect in such cases; and its presence as a factor in the genesis points to a psychological argument contrary to the initial postulate of immediate wholeness.

Since this point regarding the influence of past experience obviously involves problems of learning, we may let it serve as a transition stage, and give it some added consideration before commenting briefly on their interpretation of learning. Introspectionism has always taken the attitude that, since meaning depends on personal biography, we must get rid of it, and approach actual sensations so that their qualities and laws may be discovered in their pure form. A typical illustration is that cited by Köhler.[6] If black and white papers are so placed on opposite sides of a screen that the amounts of light reflected from them singly, are equal, we none the less call the first one 'black,' the second, 'white.' That the sensations—brightnesses of both—must be the same can be demonstrated by observing the papers through two holes in a screen, so that one hole is filled by a section of the black, the other by a section of the white paper. An introspective attitude gives similar demonstration of their brightness equality. So, too, with optical illusions, stroboscopic movement, and so forth. Introspectionists have inclined to the view that primary sensory experiences are the subject matter of psychology; the warpings of those experiences, their interpretations by means of reproduced meanings, are not to be studied. Psychological laws lie inherent in the primary bases.

But, say the Gestaltists, are we not thereby discarding most of the facts of objective experience? And is not this precisely what the Behaviourist is doing—from a different

[5] C. Spearman, Creative Mind, London, 1930, p. 144.
[6] W. Köhler, Gestalt Psychology, 1929.

point of view? Consequently they are led to an examination of the systematic bases of both introspectionism and behaviourism, and find them equally narrow. Let us see how this comes about.

Subjective attitude can change the appearance of the black and white papers. Hence, the introspectionist says, the true sensory facts must be those that are independent of such changes in attitude. But still, the return to natural observation gives even the introspectionist the black and white of the ordinary observer, and this fact demands examination. However, since the unusual experiences obtained in introspection are readily paralleled with the physical properties of peripheral stimulation, psychology has tended to accept these as true, and has been so busy with the examination of them, that it has overlooked the others. Accepting stimulation as a purely local experience (the constancy hypothesis) it has allowed this physiological god to determine what should be investigated; and direct experience as such has been so transformed before investigation, as to be no longer the object of enquiry. Further, since this physiological god (the independence of local stimulation) is based on the assumption of isolated conductivity of neural elements, the parallel between introspectionism and behaviourism is complete; acquired meanings are interpreted as similar connections at a higher level of the nervous system which did not exist at birth; the 'meaning' of the introspectionist is known to the behaviourist under the rubric 'conditioning.' Why not, however, study direct experience—in which case, say the Gestaltists, the effects of stimulation lead us away from the constancy hypothesis, and demand a dynamical interpretation, since the salient features are those of interdependence of all aspects of experience! It is a much more difficult field; but it is the field of everyday life.

But, says the behaviourist, can direct experience be postulated, let alone studied? The Gestalt reply to this question is that given by others, and need not be elaborated. The assumptions underlying behaviourism as a method are purely epistemological, and very one-sided. Discarding the concept

of experience because it involves an epistemological postulate in others, he none the less accepts uncritically the equally unprovable realism of the physicist. The Gestaltist, like other psychologists, naïvely accepts the one as well as the other. Physics has not suffered in consequence of such philosophical innocence. Psychology need not, if it follows the path of scientific development laid down by the established sciences; and that path is one of seeking and isolating problems to be investigated through qualitative study of experience. Only then will our quantitative refinements be interpretable.

As we have seen, this leads to a realization of the interdependence of all aspects of the sensory field. Instead of postulating an unexamined, acquired meaning as the obstacle to true psychological enquiry, and hence trying to get rid of it (as do the introspectionists), we should note the fact that in experience no discrete sensory units are ever found. Every aspect of stimulation is so intertwined with others, that the system as a whole is the only functional unit; and the formal qualities are the only ones that determine what our experience will be. If however, these formal qualities are such that they correspond with those of past experience, then the interpretation in terms of the past takes place. Reproduction depends upon the form of a segregated whole in the field. The form is not created by reproduction, but *vice versa*. Hence, again, the starting point of enquiry shifts, in Gestalt, to the immediately given; and from this we are confronted with certain facts demanding an hypothesis different from those used heretofore. The psychological genesis, however, is not yet depicted. It is the contention of the writer that this genesis can be examined psychologically, without necessary recourse to introspective methods; and a beginning has been made in this direction, using the devices pertinent to the field of individual differences, and based on the fundamental difference between primary and secondary perception as above indicated [7] By such means of investigation, instead of postulating neural cross-functioning or potential differences as the essence of structural phenom-

[7] W. Line, Die Lehre der Noegenesis, *Arch. f. d. ges. Psychol.*, 1929, vol. 68, pp. 241–267.

ena, and hence implying that cognition is merely epiphenomenal to cerebral patterning, the functional processes of relating are considered to be (and demonstrated as being) basic, and the term 'relation' resumes its older, wider meaning, rather than the limited connotation given to it by the Berliners. Implicit, 'as' relations are linked psychologically with explicit, judgmental, analytic, 'that' relations.[8] Apart from this, the method should parallel that of Gestalt psychology, future developments and researches being charged with the responsibility of demonstrating the relative merits of the two approaches.

In the field of memory, the organization of ideas is analogous to the unity of the sensory field. Memory traces are pictures of past organization, and as such they function in reproductive processes. But obviously association or union demands more than mere contiguity as its condition in experience. It depends on the dynamical relations between the parts. In discussing the memorization process, Gestalt writers invariably stress the need for *intending* the learning, actively relating the parts, and so forth, until an organized whole is achieved. The nature of the whole is then described in terms of its internal structure, its tensions, and the like; and reproduction is certainly a function of the whole, rather than of discrete parts. But 'synthesis' is stressed—a synthesis directed by the intention to learn, and dependent on the dynamical relations which spring up (supposedly from the discrete ideas themselves) and weld the whole together. So that we come even nearer, at this point, to a recognition of active relating, although there is no active agent. Neural physics comes in where we rather anticipate *g*. The relations are not educed; they educe themselves.

Support for this dynamical conception is drawn from such experiments as those of Wulf, or from the observations of everyday experience. The non-rigidity of memory traces shows itself in the loss of irregularities, or the accentuation of peculiarities in memory images. Of great importance, too, is the inclusion within the dynamical field, of the Self,—not as

[8] See C. Spearman, Proc. IXth International Congress of Psychology, 1930, p. 403.

a thing apart from other phases of experience, but as a factor in the total interplay of stresses. Directed attitude, intention, conation, desire, all tumble in as vectors in a force system demanding closure by its very nature. The inadequacy of associationism to justify the need for such terms as 'distraction,' is overcome by making cognitive phenomena incidental to orectic manifestations; and Lewin's work looms as the evidence for the importance of the tensions set up between the self and the goal to be achieved. When the experience is patterned so that certain aspects of the pattern are felt directly to depend on other aspects, the term *insight* is applied. Insight is the *why* and *how* of a situation,—an understanding of the innermost nature of the field, the stresses not merely apprehended but comprehended. It is difficult to conceive of this as pure dynamics, since the self appears to be given the attributes of activity and knowing (although 'insight' is more usually expressed by the Gestaltists as a 'feeling,' almost an intuition rising out of the nature of the situation.)

What does this imply for the field of learning generally? It immediately suggests that if we are to direct the learning of others, the tasks set must be articulated with the nature of the learner. There is nothing new in this. But if we examine learning in cases where such articulation is not considered, we become involved in aspects other than those which we intend to study. The importance of repetition, for example, has been stressed in a manner that leads to misinterpretation of its function. Motives, emotions, punishments, and the like, tend to be viewed as conditions apart from, though connected with the learning process, whereas, for Gestalt, they are aspects of the process itself. The perception of a goal is an indication of the process in its beginnings, and physically implies a force distribution or upset equilibrium demanding closure,—a re-organization of the animal being accomplished in the process. We may intend that the animal achieve a certain result. Our method must be such that his energising is directed to secure that organization; and one of the tricks of the trade is to arrange the situation so that the appropriate goal is intended by him. This we call motivation. Such an

approach puts the emphasis in learning on the experience side of the picture. To this extent it is in harmony with older experientialist doctrines and against the mechanics of trial and error and reflex summation theories. The primary fact is organization everywhere, not combination.

In conclusion, the main trend of this paper may be reviewed. It has tried to show how far the Gestalt movement seems to indicate novel problems, or at least to suggest novelties of emphasis and approach, even though the problems are closely allied to those of past thought. A promising start has been made towards a less artificial, less abstract, perhaps less naturally scientific, approach to problems of behaviour, of human living. Its accomplishments are necessarily few, save in the matter of pointing out defects inherent in many current theories and methods. Until it has made its position more clear, more definitely psychological, more readily demonstrable, it will perforce be concerned with negative aspects. When it develops more positively, when its point of view is applied to a wider range of behaviour situations, it will be more easy to evaluate its contribution. Some scientists suspect that when it does this, its systematization will be possible only in so far as the analytic methods of other branches of psychology are brought into play. But at present one feels that, since there is nothing in Gestalt that is radically opposed to other formulations of experiential psychology, its worth should be a matter for the future, so that healthy rivalry rather than one-sided enthusiasm or antagonism will result. It is this attitude which gives rise to the feeling that some aspects of present-day attitude are disconcerting for, to write an Educational Psychology, or even a General Psychology on the basis of so uncrystallized a system, seems to be somewhat imprudent, were it not for the fact that current standards in such fields furnish very little competition.

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IN DEFENSE OF SOME DISCARDED CONCEPTS [1]

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My early training in psychology dates back to what may be termed the Neolithic period. The texts generally assigned to the American undergraduate in the late 1880's were Bain and Sully of the British School, or our home products, Hickok, Porter, and McCosh. These authorities differed considerably in standpoint and treatment, and their differences were often acrimoniously emphasized. But they were at one in upholding the introspective method of investigation, and in limiting psychology to the treatment of conscious experiences. Bain followed the traditional view of Hartley and James Mill, which attributed complex experiences to the association of ideas. McCosh favored the Scottish intuitionism of Thomas Reid. Both schools agreed in assigning a prominent rôle to sensation and volition. And there was a general disposition to classify mental phenomena into three groups, variously known as cognition, affection, and conation, or as intellect, feeling, and will.

Early impressions are not easy to eradicate. In spite of the revolutionary changes wrought in psychology by quantitative laboratory methods, by the experimental investigation of animals and children, and by pathological studies, I still believe that many of the older findings are valid and useful. The introspective method, though vigorously challenged by the behaviorists, furnishes clues to certain intra-organic phases of the stimulus-response operation which are not yet accessible by any other means. The principle of the conditioned response has already substantiated to some extent the cardinal tenet of the English school, by interpreting association in neural terms.

[1] Address of the Honorary President before the New York Branch of the American Psychological Association, Columbia University, April 18, 1931.

Certain other fundamental concepts of pre-experimental psychology have been less fortunate. The rôle of sensation has been minimized, and emphasis is now laid on the activity of the organism as a whole. The historic classification of mental phenomena or functions has fared worse. Even when such terms as cognition, affection, and conation are used, they are seldom regarded as representing a basal distinction.

I propose today to file a brief in behalf of these two discarded concepts: (1) I wish to point out the fundamental importance of Sensation—the reception of data by the organism as an individual; and at the same time to emphasize the central control of the organism's responsive activity—that factor which the older psychologists hypostasized as Will. (2) I shall defend the factual basis of the three-fold classification into Cognition, Affection, and Conation, or whatever equivalent terms we choose to adopt.

I. Sensation and Volition

The dethronement of sensation has been due to a variety of causes. The Introspectionist today recognizes that even our most elementary data are complex. Every actual conscious experience includes a variety of constituents, which are juxtaposed, fused, and modified by the residual of earlier experiences. Analysis of an experience into simple sensations is accordingly not a matter of direct observation. It can only be made indirectly, by reference to the external sources of the experience; and this involves the so-called stimulus error. Much the same conclusion is reached by the study of behavior or by examining the neural aspect of the receptive process. The significance of any single stimulus or any single nerve impulse is lost in the composite activity of the organism as a whole.

Now it seems to me that we have carried too far this concept of the organism acting as a whole. Not but that it is extremely important to emphasize the unitary character of organic processes. Organs, members, cells, interact. Physiological functions work together, sometimes simultaneously, sometimes serially, as part of a single organic operation. In

the domain of psychological interest, stimulation of the receptors is of value to the creature only as it is related to the response process. What I question is the usefulness of attempting to link the activity of each specific physiological operation to every part of the organism.

The interplay of forces within the organism is not essentially different from the interplay of physical forces in the universe. It is not improbable that the fall of a stone on the earth is somewhat affected by the position of the moon—perhaps even by Sirius. But the quantitative value of this effect is infinitesimal compared with that of the forces in the immediate neighborhood. The physicist is justified in ignoring these ultra-microscopic concomitants in dealing with the problem of a falling body on the earth. In the same way, when we are dealing with problems of cell growth or responsive behavior, our interest lies only in the truly significant factors and relations involved. We know that at any given instant a multitude of stimuli act upon the individual's various receptors. The nerve impulses generated by such stimulation radiate through many paths and by-paths and result in a tremendously complex neural pattern. Nevertheless it seems questionable whether all, or even a large proportion, of the several billion neurons in the brain are significantly affected by any momentary situation. In certain instances, where powerful stimuli are acting, the resulting neural patterns may be hopelessly intricate. Yet even so there undoubtedly remains a large quiescent background, which need no more be considered in this connection than the effect of the moon's position in the problem of the falling stone.

The real value of the organismic conception of activity lies in its unitary character, rather than in its pervasiveness. We must treat the organism as an interacting system. The cooperation of its functions is far more important than the separate functions themselves. But to insist that every organic function is concerned in the working of each specific physiological activity, or that the entire nervous system takes part in each stimulus-response operation, seems an unprofitable and unwarranted assumption.

Psychology is particularly concerned with certain specific types of organs, which form a subsystem within the organism. The receptors, the nervous system, and the effectors (including glands, muscles, and mobile parts of the body) are its chief structural data. This group or chain of organs constitutes a well-ordered mechanism. Each type of organ has its own specific type of function. But under normal conditions these organs operate together as an articulated system. The psychologist is interested only incidentally in the separate functions of stimulation, conduction, muscular contraction, and the like. He is concerned, rather, with the operation of the whole group as a system.

Even this is an incomplete characterization of the scope of psychology. The outcome of muscular contraction is bodily movement; and this brings about an adjustment of the relation between the organism and the environment. This end-result, adjustment, is the distinctive factor in the psychologist's problem. In other words, the function of psychology, as I interpret it, is to investigate the adjustment of the creature to situations which confront him, and the means by which this adjustment is brought about.

From this standpoint psychology may be regarded as a biological science, since it is concerned with certain life-maintaining processes of the organism. And it is distinguished from physiology in that it studies adjustment as a unitary operation, while physiology is concerned with the separate bodily functions.

This is indeed a radical shift from the introspective standpoint of Bain and McCosh. But it should be remembered that even the traditional psychology recognized some sort of relationship or correlation between conscious experience and the activity of sense-organs, nerves, and muscles. Today the relative importance of consciousness and neural activity has become inverted. But the correlation still remains apparent. For this reason I do not see how we can legitimately exclude introspective data from the scope of psychology, though they have now become a relatively unimportant factor in a much broader problem.

In spite of this complete reconstruction of psychology, there is something in the general standpoint of traditional psychology that is well worth considering. For the concept of the organism as a system points to a treatment of the major problem of psychology which is quite analogous to that of the earlier writers. We can view the organism—the creature—as receiving sensory data or impressions through the receptors, and as acting responsively upon these data. Now this is virtually the standpoint of the traditional psychology, except that these impressions were supposed to operate upon a hypothetical soul-substance, and the individual's responsive expressions were attributed to the initiative or will of this entity. Apart from that metaphysical assumption, the traditional view harmonizes with present knowledge.

Moreover, it seems to me that there is an essential difference between this standpoint and that of the current behavior psychology. The objective treatment of the psychological problem which is prevalent today rests on the concept of a *stimulus* leading to a *response.* For this conception I would substitute the idea of a *situation* leading to an *adjustment.* There is actually a great disparity between the total physical environment and the situation to which the organism adjusts itself. The dynamic pattern of the physical environment becomes a situation for the organism only in so far as certain forces stimulate the creature's receptors. Hence, a prime requisite for any response is sense-impression. Each separate stimulus makes an impression upon the organic system and thereby contributes its share to the ensuing response. The older psychology regarded these elementary effects of stimulation as directly correlated with elementary conscious experiences, ignoring possible complications within the nervous system. The term *sensation* is therefore ambiguous. It may mean either the elementary unit of consciousness, or the elementary excitation of the sense organ. This confusion is avoided if we substitute the term *impression* for the elementary effect on the receptor.

But what we are considering here is the question of standpoint rather than terminology. The objective psychologists

emphasize the *stimulus* which acts upon the organism. The traditional psychology emphasizes the *impression* which the creature receives from that stimulus. And if we recognize the unitary character of the organism the latter standpoint seems preferable.

The efferent and motor processes may be interpreted in the same way. If we conceive of the organism as a unitary system, then responsive activity is the organism expressing itself, and the result of this expression is an adjustment.[2] This notion of self-expression is fairly equivalent to the traditional view, if we discard the metaphysical implications of the term *volition*. It must be remembered that traditional psychology was firmly tied up to the concept of soul-substance. Even today *volition* popularly suggests an unguided initiative, and probably for that reason is looked upon askance by many psychologists. The term *expression* is not open to this objection. I would be among the last to advocate any portrayal of the stimulus-response process that gave comfort to the soul-substance metaphysics. The dynamic configuration or pattern of the nervous system at any given moment determines the specific response. Whether we interpret this central factor wholly in terms of neural processes, or take into account the experimental concomitants as well, makes little difference. It is the central system that controls the creature's responses. The stimuli which act upon the receptors have no significance for psychology till this activity is communicated to the central system. The responses that follow neural activity are adaptive because the efferent impulses have been coordinated by the central nervous system. Response, then, may be regarded as the organism expressing itself.

And so, in both the initial and final phases of the situation-adjustment process we are drawn back to the standpoint of traditional psychology, though not to its metaphysics. Psychologically the terms *impression* and *expression* are preferable to *sensation* and *volition*, in my opinion at least. But we are interested in standpoint rather than terms. The traditional

[2] An adjustment may or may not be biologically useful (*i.e.* adaptive).

psychology was subjective; behaviorism is objective. The standpoint I am defending is to some extent a compromise between the two.

The behavior psychology, in its effort to escape subjectivism, seems to have wandered too far in the other direction. It has not taken sufficiently into account the rôle of the organism. The very term *stimulus-response* indicates this. In studying behavior the experimenter gives a stimulus and notes the result, without regard to the important intervening processes. When we recognize the organism as a system, our standpoint is shifted. Instead of picturing the organism as being stimulated, we say it receives an impression; instead of picturing the response as simply the effect of stimulation, we regard behavior as an expression by the organism, carried into effect through its own neural activity.

The language used by traditional psychology in describing these processes was naïve and anthropomorphic. The functions or faculties were personified, just as the Freudian psychology has personified the censor and the subconscious. Today, personality is seen to be the outgrowth of life-long experiences and behavior. To that extent our standpoint differs from the traditional. But while the soul-substance of medieval metaphysics has been cast out of psychology, the organic system remains in good standing. It represents the reference frame which psychologists adopt in their investigations. The axial zero-point of this frame is the nervous system. The older psychology was egocentric; the psychology of adjustment would most fittingly be described as organocentric or neurocentric. To this extent the older standpoint seems preferable to pure objectivism.

II. Cognition, Affection, and Conation

There is still another doctrine of traditional psychology which seems to have been unwarrantably abandoned. In the current American texts we rarely find a basal division of topics corresponding to the historic Cognition, Affection, and Conation. Even Titchener, who limits psychology to experiential phenomena, lists in his *Text-Book* as major topics, Sensa-

tion, Affection, Attention, Perception, Association, Memory and Imagination, Action, Emotion, and Thought. In my own texts, the three-fold division is definitely pointed out, but it is not emphasized, and may easily be lost sight of in the later ramifications of the subject-matter. Yet this discovery by the earlier psychologists seems to me to have been one of their most important contributions,—ranking probably next to their development of the laws of association.

William McDougall has endeavored to bring this introspective trichonomy into line with the neural data in his theory of instinct. According to McDougall the cognitive aspect of experience corresponds to the afferent segment of the nervous arc, the affective to the central, and the conative to the efferent segments.[3] This correlation is rather tempting, especially as applied to cognition and conation. But the assumption that affective phenomena are specifically related to modifications of neural activity that occur in the central segment of the arc presents difficulties. For affective experiences seem actually to be based upon characteristics of certain afferent data. They are not merely due to central integration. They indicate the intra-organic phases of the total situation which confronts the creature at the moment. Moreover, the correlation between conation and outgoing nerve impulses seems doubtful. The earlier concept of centrally initiated innervation feelings, which gave color to an afferent interpretation of conation, has long since been abandoned in favor of kinesthesis. Thus McDougall's theory, which rests on a purely introspective basis, does not seem to harmonize with the neural facts.

But there is another interpretation of the threefold introspective classification, which meets these objections and which may prove serviceable in connection with our study of the intermediate or central operations of the stimulus-response chain.

We are indebted to the physiologists for introducing the term *receptor* in place of the older term *sense-organ*—a valuable acquisition indeed, since it permits one to discuss the problems

[3] W. McDougall, Social psychology, 1st ed., pp. 32 ff; 14th ed., pp. 33ff.

of responsive adjustment either with or without regard to introspective material. A far more important contribution to the terminology and conception of the receptive process was made in 1906 by Sherrington, when he suggested the terms *exteroceptor* and *interoceptor* to distinguish two different groups of receptors.[4] The exteroceptors are those organs which receive stimuli from the external environment—the interoceptors those which receive stimuli from within the organism itself. Now it is obvious that the external situations which a creature faces, call for a totally different type of response from the responses demanded by internal situations. External stimuli may be gotten rid of or modified by movement; internal stimuli accompany the creature wherever he goes.[5] There is thus a valid objective basis for distinguishing between these two groups of receptors. Introspection also would seem to indicate that affective experiences, or the affective components of experience, are due to data derived from the internal receptors; whereas those experiences which are called cognitive are largely traceable to stimuli received from the environment. If, then, the distinction between exteroceptors and interoceptors has a real behavioristic significance, it appears to be no less important to introspective psychology as furnishing a basis for the distinction between cognitive and affective experience.

At the same time Sherrington proposed a third term, *proprioceptor*, which he applied to the receptive organs situated in the muscles and other mobile structures.[6] The muscle sense or kinesthetic sense had already been recognized by psychologists. But it is largely to Sherrington's work, and researches suggested by his studies, that we owe our present knowledge of the rôle of kinesthetic stimuli and the resulting afferent impulses in coordinating and regulating muscular response-movements. What I wish to call attention to, however, is that *proprioceptor* is a generic and not a specific term. It is applicable not merely to the receptors in the

[4] C. S. Sherrington, On the proprioceptive system, especially in its reflex aspect, *Brain*, 1906, 29, 467–482 (469, 470).

[5] Cf. E. B. Holt, Animal drive and the learning process, ch. 14, esp. pp. 125, 133.

[6] C. S. Sherrington, On the proprioceptive system, *Brain*, 1906, 29, 472 ff.

contractile tissues, but to any specific receptor that fulfills a certain type of function. Any receptor which furnishes data regarding the relation of various members or parts of the body to the general configuration of the body or to the environment, or data regarding changes in these relations, is a proprioceptor. For this reason, as Sherrington points out, the static receptor in the ear may be included in the proprioceptive group.

The terms *exteroceptor* and *interoceptor* are also generic. The exteroceptive group includes a number of well-recognized sense-organs; in fact, the five senses noted by the earlier psychologists all belong to this class. The receptors associated with the organs of digestion and other autonomic mechanisms belong to the interoceptive group. Their buried position renders them less accessible to experimental investigation than the external receptors, so that it still remains uncertain how many of them are structurally or qualitatively distinct. But their rôle in the situation-adjustment process is identical: they furnish data regarding intra-organic conditions. Furthermore, if we assume as a general principle that qualitative differences in conscious experience are determined by afferent data, then that vague, indefinite experience called *feeling*, including both pleasantness and unpleasantness (Lust and Unlust), may also be attributed to interoceptive activity.[7]

The significance of Sherrington's contribution in its bearing upon introspective data has not received the attention it deserves. This is perhaps due to his special treatment of the *nociceptors*, which might be regarded as a fourth type.[8] There

[7] It should be noted that Herbert Spencer, in his Psychology, also proposed a three-fold classification of primary experiences or 'feelings' based upon the mode of reception. He divides them first of all into centrally initiated and peripherally initiated. The centrally initiated feelings are the emotions. The peripherally initiated feelings he further divides into two classes, according as they are 'caused by disturbances at the end of nerves distributed on the outer surface . . . [or] by disturbances . . . within the body' (Principles of Psychol., 1880, 3d ed., sec. 66). The former group, which he calls *epiperipheral* feelings (sec. 112), imply external agents or stimuli. The internally initiated, which he calls *endoperipheral* feelings, include sensations of muscular tension, pain, and hunger (sec. 68). It is evident from these examples that Spencer's classification does not correspond to Sherrington's.

[8] C. S. Sherrington, Integrative action of the nervous system, 226 ff. *Cf.* his art. in *J. Physiol.*, 1903, 33, 39–46.

seems to be no ground for treating the nociceptors as a separate genus of receptors. Pain (or injury) stimuli really perform the same office as the interoceptive stimuli generally. They furnish data as to the internal condition of the organism; and the type of response to such stimuli, though usually more vigorous than other interogenic responses, is generically the same.

A pain sensation indicates maladjustment to certain features of the internal situation. The stimuli may be either deep-lying or peripheral, but they are always part of the organism. Even when the stimulation occurs through the operation of external objects, the conditions which it portrays are intra-organic. The sensory data that arise from cutting, bruising, burning, pricking, and the like, are only indirectly attributable to the environmental situation; actually they are due to the injury which the external agent inflicts upon the creature's bodily tissue. If you are cut by a sharp knife or burned by contact with a hot radiator, the pain sensations give you no added information about the knife or the radiator. They tell of bodily disorder, just as truly as does a tooth-ache or a stomach-ache. There seems no reason for regarding this mode of receptivity as other than interoceptive.

We are led, then, to the conclusion that the traditional distinction between cognitive, affective, and conative experiences may be traced to differences in the type of situation indicated by the receptive data. A creature responds to three distinct kinds of situation: (1) the external situation, that is, the play of environmental forces upon his body; (2) the internal or intra-organic situation; that is, his state of nutrition, the condition of his genital organs, the degree of fatigue, exhilaration, maladjustment or injury, and the like within his own body; (3) the motor and static relations between his body and the environment, and between certain members of the body and the body as a whole. To each of these three types of situation a radically different type of response is appropriate.

The modus operandi of these different types of response remains for the neurologist and behaviorist to trace out. But it is a significant fact that introspective analysis had discov-

ered a correlate of these three types in conscious experience long before the experimental study of behavior and of nerve impulses began. The cognitive phenomena of experience are based largely on data received through the external senses; affective phenomena on the internal or systemic senses; and conative phenomena on proprioceptive data.

A few exceptions to this generalization should be noted. Kinesthetic sensations furnish data not only regarding our active movements and the tension of our members, but also as to the weight of lifted objects, which is a cognitive phenomenon. And there are esthetic judgments in which interoceptive data play an important part. But in the main the sensory data on which our intellectual experiences are based are of exteroceptive origin. We need not insist upon a rigid schematization. Even the distinction between organism and environment is not always clearly marked—as for instance in the case of food which is in process of digestion, and of the scab that covers a wound. The significant point is that there are three distinct phases of the situation with which the organism is at any moment confronted, and to which the organism adjusts itself by three different types of response. There are also three distinct types of conscious phenomena, the cognitive, affective, and conative, which were brought to light by introspective analysis. And there is, in general, a one-to-one relation between the three members of these two groups.

When we stop to consider, it is not strange that the early investigators, however crude their method now appears, should have discovered this fundamental fact of mental life. For if the phenomena of conscious experience have any real significance, it is on account of their close relation to organic activity. The data of conscious experience are received through the sense organs and reveal the situation confronting the individual. Fundamental differences between various types of situation would obviously affect the content of conscious experience as well as the creature's mode of behavior.

All this suggests that possibly other introspective findings deserve special consideration at the hands of psychologists, to

determine their bearing upon neural processes and behavior. Attention, for example, must surely have some important neural significance. It is a phenomenon observed introspectively, and its end-results are observable in behavior. We may well inquire what characteristic or operation of the nerve impulse is involved in Attention. And so of Memory, Fatigue, Emotion, and other phenomena or factors of consciousness. Each of these traditional introspective terms seems to embody a real problem for present-day psychology.

Take a concrete illustration. The play of after-images, or after-sensations, positive and negative, is usually limited to a very few minutes, unless the stimulus be exceedingly intense. But there are instances of delayed visual after-images which have appeared spontaneously a half hour or more after the stimulating situation has passed away. I have reported two such cases from my own experience.[9] In each case the interval was spent in sleep. Recently a delayed series of after-images appeared spontaneously 30 minutes after the experience with which I identified it, during which period I was fully awake.[10]

Are these delayed after-affects due to some latent retinal condition? Or are they centrally aroused? Their vividness certainly points to a peripheral origin. But how can we answer definitely till we know more about retinal physiology and cortical activity? [11]

[9] H. C. Warren, Some unusual visual after-effects, PSYCHOL. REV., 1921, 28, 453–463; A delayed visual after-effect, *Amer. J. Psychol.*, 1929, 41, 684.

[10] The after-effect was the picture of a wooden fence, with a narrow strip of dark ground at the base, and bright green grass filling the lower part of the field. It moved from left to right at a rate corresponding to my speed in walking. The scene appeared identical with the neighboring yard which I had passed on returning home in bright sunlight, 30–40 minutes earlier. Meanwhile I had lunched in a room of medium brightness, and then lay down to rest. On closing my eyes the after-effect followed within a minute.

[11] I should take decided exception to the following attitude: "Tout autre est le cas d'un homme qui s'observe lui-même, car il connaît à chaque instant l'état de son système nerveux. Il ne le connaît pas, il est vrai, de manière à pouvoir en faire une description histologique; il peut même ignorer jusqu'aux caractères les plus grossiers de la structure anatomique de son cerveau et, cependant, il en a une connaissance *extrêmement précise*, quoique cette connaissance ne se traduise pas pour lui d'une manière visuelle." F. Le Dantec, Les influences ancestrales, 1920, pp. 77–8.

Our criticism of the behavioristic movement is not for its rigid adherence to its own particular method of research, but for its failure to recognize the importance of introspective and neurological study. Investigation of the response is extremely useful. But the intermediate processes in the stimulus-response chain are far more significant. It is these operations that determine the specific response. Introspective research, old and new, has furnished many clues as to the nature of these intermediate processes. One great mistake in the past was the attempt to interpret introspective data in neural terms before the real character and workings of the nerve impulse were discovered. Only patient experimental research upon the nervous system can reveal the nature of neural activity. Introspection can not do so. But at least it can formulate certain fundamental problems of the adjustment process and point out some of the more promising directions of research.

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MULTIPLE FACTOR ANALYSIS

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The two-factor problem of Spearman consists in the analysis of a table of intercorrelations for the discovery of some general factor that is common to all of the variables in the table. Spearman differentiates three types of factors, namely, a general factor which is common to all of the variables, group factors which are common to some of the variables but not to all of them, and specific factors that are peculiar to single variables alone. In practice, the Spearman two-factor methods meet with the difficulty that group factors are frequently encountered. The two-factor methods are not applicable to situations that involve group factors except in indirect ways. This is a serious limitation on Spearman's technique since many important psychological problems involve a complex of variables that are known from the nature of the problem to contain group factors. The present multiple factor methods in no way contradict the Spearman two-factor methods which are very ingenious and powerful in the situations to which they apply. The present multiple factor method may be thought of as supplementary to the Spearman two-factor method in that we do not have any restrictions as to the number of general factors or the number of group factors.

It is the purpose of this paper to describe a more generally applicable method of factor analysis which has no restrictions as regards group factors and which does not restrict the number of general factors that are operative in producing the intercorrelations. Our first question concerns the number of general, independent, and uncorrelated factors that are operative in producing a given table of intercorrelations for any number of variables. In our terminology general factors will include what Spearman calls general and group factors.

This question can be answered by methods that will here be described and which are applicable to any table of intercorrelations. We may consider three examples to illustrate the nature of this first problem. If we have a table of intercorrelations for a battery of motor tests it is of considerable psychological interest to know how many independent motor abilities it is necessary to postulate in order to account for the whole table of intercorrelations. If this turns out to be three, then our next task would be to hunt about for the nature of these three motor abilities. If we have a table of intercorrelations of the interests of eighteen professions it would be of considerable importance to know how many independent interest factors it is necessary to postulate in order to account for the whole table of intercorrelations. This refers to the tables published by E. K. Strong. We have applied our methods to his data and we have found that his table of intercorrelations can be accounted for by postulating four general interest factors which turn out to be (1) interest in science, (2) interest in language, (3) interest in people, and (4) interest in business. Again, Professor Moore [1] has prepared a table of intercorrelations of 48 psychotic symptoms on the basis of his work with about four hundred patients with various psychoses. A general factor analysis of the type here discussed would enable us to know how many general factors or mental disease entities it is necessary to postulate in order to account for the whole table of correlations of psychotic symptoms. If this should turn out to be five, for example, then we should be justified to look for five fundamentally different psychoses.

Our next problem is to assign a weight or loading of each of the general factors to each of the variables. For example, in the table of interest-correlations above referred to we should assign four loadings, one for each of the four general factors, to each of the eighteen professions. It then turns out that Engineering, for example, has a high loading of interest in science, a rather low loading of interest in language. The profession of law has just the reverse loadings, namely low

[1] T. V. Moore, The empirical determination of certain syndromes and underlying praecox and manic depressive psychoses, *Amer. J. Psychiat.*, 1930, **9**, 719–738.

for science and high for language. The ministry is loaded high for interest in people and in language but low for science. Finally, we should want to be able to assign to each individual subject a quantitative rating in the form of a standard score for each of the general factors or abilities that have been isolated.

Let there be n factors. In this explanation n will be assumed to be three. It can be any number.

Let there be N individuals in a group, all of whom have taken w tests.

Let a, b, c, d, etc., represent the tests.

Let the three factors be represented by numerals 1, 2, 3.

S_a = standard score of one individual in test a.

S_b = standard score of one individual in test b.

S_c = standard score of one individual in test c.

$S_a = \dfrac{X_a - m_a}{\sigma_a}$ = the usual definition of a standard score.

The standard score S_a of an individual in test a depends on (1) his rating in each of the three abilities or factors 1, 2, 3, and (2) the weight or loading of each of these abilities in test a. For example, if test a calls for much of ability No. 1 and very little of abilities 2 and 3, and if one subject has a low rating in ability No. 1 and average or high ratings on the other two abilities, then this subject may be expected to do poorly on test a. The loadings of the three general factors in each test and for each subject may be represented by the following notation.

Let

x_1 = standard score of an individual in ability No. 1.

x_2 = standard score of an individual in ability No. 2.

x_3 = standard score of an individual in ability No. 3.

and let

a_1 = loading of ability No. 1 in test a,

a_2 = loading of ability No. 2 in test a,

a_3 = loading of ability No. 3 in test a.

Then we shall assume that the standard score of each individual subject is a sum of the products of his standard score in each ability and the loading of the ability in each test. This assumption leads to the following fundamental equations.

$$S_a = a_1x_1 + a_2x_2 + a_3x_3 \qquad (1)$$

and

$$S_b = b_1x_1 + b_2x_2 + b_3x_3.$$

Strictly speaking, we should add to each of these two expressions a term to account for those additional general factors, beyond three, which are here ignored, and also a term to account for the specific factor, peculiar to the particular test. However, our object is to ascertain how many general and independent factors it is necessary to postulate in order to account for a whole table of intercorrelations and we shall therefore intentionally ignore these additional specific factors as well as those minor group factors which may not appreciably affect the correlations.

We want to express the correlation between tests a and b in terms of the standard scores in the three abilities and the loadings of the three abilities in each of the two tests. For this purpose we shall need the product S_aS_b. Then

$$S_aS_b = a_1b_1x_1^2 + a_2b_2x_2^2 + a_3b_3x_3^2 + \text{cross products.}$$

The correlation r_{ab} can be expressed simply as

$$r_{ab} = \frac{\Sigma S_a \cdot S_b}{N},$$

because S_a and S_b are both standard scores so that the standard deviations of the given scores are all unity. Then

$$\frac{\Sigma S_aS_b}{N} = a_1b_1\frac{\Sigma x_1^2}{N} + a_2b_2\frac{\Sigma x_2^2}{N} + a_3b_3\frac{\Sigma x_3^2}{N},$$

in which the cross products vanish because x_1, x_2, x_3 are uncorrelated. But

$$\frac{\Sigma x_1^2}{N} = \frac{\Sigma x_2^2}{N} = \frac{\Sigma x_3^2}{N} = 1,$$

since x_1, x_2, x_3 are all standard scores in the three abilities. Therefore

$$r_{ab} = a_1b_1 + a_2b_2 + a_3b_3. \qquad (2)$$

This is one of the fundamental equations. Here the correlation between two tests is expressed in terms of the loadings of the three abilities in the two tests. By analogy we may write equation (2) for any pair of tests, as

$$r_{ac} = a_1c_1 + a_2c_2 + a_3c_3$$

and so on.

Another fundamental equation can be derived as follows:

$$S_a = a_1x_1 + a_2x_2 + a_3x_3.$$

Squaring,

$$(S_a)^2 = a_1^2x_1^2 + a_2^2x_2^2 + a_3^2x_3^2 + \text{cross products.}$$

Summing and dividing by N

$$\frac{\Sigma S_a^2}{N} = a_1^2\frac{\Sigma x_1^2}{N} + a_2^2\frac{\Sigma x_2^2}{N} + a_3^2\frac{\Sigma x_3^2}{N}.$$

The cross products vanish because x_1, x_2, x_3 are uncorrelated by definition. But

$$\frac{\Sigma x_1^2}{N} = \sigma_1^2 = 1$$

and

$$\frac{\Sigma S_a^2}{N} = 1 \text{ by definition.}$$

Hence

$$a_1^2 + a_2^2 + a_3^2 = 1, \tag{3}$$

and similarly by analogy for every other test, as

$$b_1^2 + b_2^2 + b_3^2 = 1.$$

Equations (3) come about because x_1, x_2, x_3 and S_a, S_b are standard scores.

Still another fundamental equation that we shall need can be derived as follows:

$$\begin{array}{rcccccc}
r_{aa} = & a_1^2 & + & a_2^2 & + & a_3^2 & = 1. \\
r_{ab} = & a_1b_1 & + & a_2b_2 & + & a_3b_3 & \\
r_{ac} = & a_1c_1 & + & a_2c_2 & + & a_3c_3 & \\
r_{ad} = & a_1d_1 & + & a_2d_2 & + & a_3d_3 & \\
\cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\
\hline
\Sigma r_{ak} = & a_1\Sigma k_1 & + & a_2\Sigma k_2 & + & a_3\Sigma k_3 &
\end{array} \tag{4}$$

In the summation equation Σk_1 = sum of all loadings of the first factor in all of the tests of the set. The notation k refers to each of the tests in succession, *i.e.*, k takes values from 1 to w when there are w tests in the series. But this summation equation may be written for each one of the tests as follows

$$\begin{aligned}
\Sigma r_{ak} &= a_1\Sigma k_1 + a_2\Sigma k_2 + a_3\Sigma k_3 \\
\Sigma r_{bk} &= b_1\Sigma k_1 + b_2\Sigma k_2 + b_3\Sigma k_3 \\
\Sigma r_{ck} &= c_1\Sigma k_1 + c_2\Sigma k_2 + c_3\Sigma k_3 \\
&\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\
\hline
\Sigma r_{kk} &= (\Sigma k_1)^2 + (\Sigma k_2)^2 + (\Sigma k_3)^2 \qquad (5)
\end{aligned}$$

in which

Σr_{kk} = sum of all intertest correlations in the whole table. Since the full table is symmetrical it follows that each intertest correlation enters twice in this sum. Also, the full table of intercorrelations includes the correlation of each test with itself and this is here regarded as unity.

Σk_1 = sum of all loadings of factor No. 1 in all tests.

Σk_2 = sum of all loadings of factor No. 2 in all tests.

Σk_3 = sum of all loadings of factor No. 3 in all tests.

Note that equation (3) is the equation of a sphere and that a_1, a_2, a_3 are the three coördinates of a point on the surface of the sphere. The space order of the sphere is equal to the number of postulated general factors. If five factors were postulated we should have a five-dimensional sphere, and so on.

Also note that each test is here really a point on the surface of a sphere. Hence, if there are three factors essentially operative in producing the intercorrelations, then it should be possible to locate each test as a point on the surface of a ball.

Note also that each correlation is the cosine of the central angle subtended by the two points a and b. Hence, if three factors are operative, then each test would be represented by a point on the surface of the ball, and the points would be

so allocated that the cosine of each central angle is equal to the intercorrelation between the respective pair of tests. This will be shown in more detail later.

Our problem is to ascertain the coördinates a_1, a_2, a_3 for test a, the coördinates b_1, b_2, b_3 for test b, and so on for each of the w tests in the whole series. Then, if our determinations are correct, it should be possible to calculate the correlation coefficients by equations (2). These calculated coefficients should agree with the observed correlations within reasonable experimental error.

If we had all of the tests allocated to as many points on the surface of a ball we could not determine the coördinates for any of these points without first deciding where our coördinate axes are to be drawn. The location of these axes is arbitrary and not at all given by the intercorrelations because the latter are merely the cosines of the angular separations between all pairs of points on the surface of the ball.

One simple plan would be to draw the axis OX through one of the tests which might be more or less arbitrarily chosen, such as a. Then if this x-axis represents the first factor, it follows of course that the xyz coördinates of point a are $(+1, 0, 0)$ and hence that

$$a_1 = +1, \quad a_2 = 0, \quad a_3 = 0.$$

The y-axis must of course be at right angles to the x-axis but it could be drawn through the origin in any direction in the plane at right angles to the x-axis.

We might now revolve the sphere around the x-axis until any arbitrarily selected second test b lies in the xy-plane. Then it is clear that the z-coördinate of point b must be zero so that b_3 in equation (3) vanishes. Therefore equation (3) becomes

$$b_1^2 + b_2^2 = 1$$

and

$$a_1 = 1.$$

The correlation between a and b is

$$r_{ab} = a_1b_1 + a_2b_2 + a_3b_3,$$

but since $a_1 = 1$, $a_2 = 0$, $a_3 = 0$, $b_3 = 0$, this reduces to

$$r_{ab} = b_1.$$

But

$$b_1 = \cos \phi$$

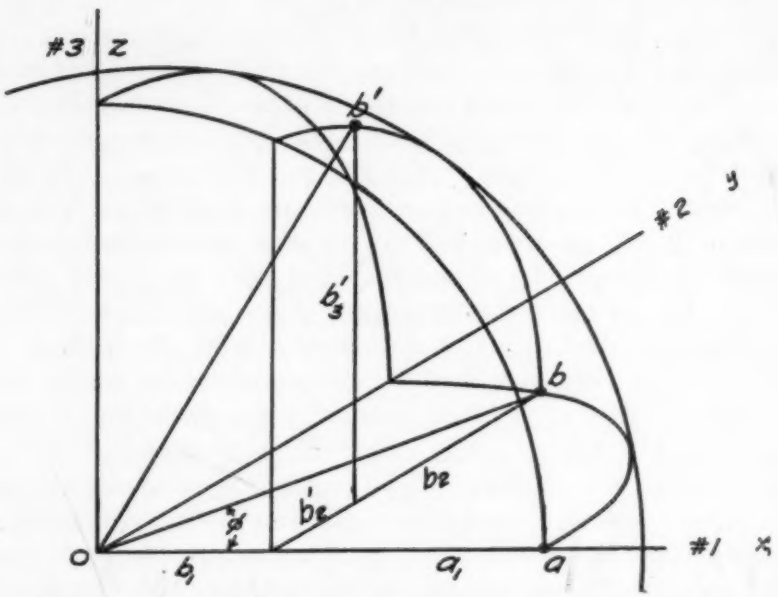

FIG. 1. This diagram shows the relation between the correlation coefficient and the central angle between the two tests or points. Let two points which represent any two tests be designated a and b'. Let the x-axis, representing the first general factor, pass through the point a. Then its coördinates are (1, 0, 0). Therefore $a_1 = 1$ while a_2 and a_3 are both zero. Revolve the sphere about the x-axis so that the point b' is in the horizontal xy-plane at b. Then the z-coördinate of the point b is evidently zero while $b_1^2 + b_2^2 = 1$. Let the angle aob be designated ϕ. Then, clearly, $\cos \phi = b_1$ since the radius of the sphere is unity. The correlation between tests a and b can be written as follows.

$$r_{ab} = a_1b_1 + a_2b_2 + a_3b_3.$$

Since a_2 and a_3 are both zero, the second and third terms vanish and since $a_1 = 1$, it follows that $r_{ab} = b_1$. The angle aob is equal to the angle aob'. Hence the correlation coefficient is equal to the cosine of the central angle between the points that represent the two tests.

and hence the correlation between tests a and b is the cosine of the central angle between them. Since the sphere can be revolved in a similar manner for any pair of tests we see that the correlation coefficient for any pair of tests is the cosine

of the central angle between them. This is true for any number of factors that may be postulated and hence for any space order.

Our problem can now be restated as follows. We have a table of intercorrelations which are cosines of central angles between pairs of points. Every test is represented by a point on the surface of a sphere. Since all of the correlation coefficients are subject to experimental errors, we cannot trust the accuracy of any one of them. Our problem is to find the best fitting allocations of the points on the surface of the sphere. The least square methods are here altogether too unwieldy. They are out of the question, undoubtedly. We shall therefore apply an adaptation of the principles of curve fitting by the method of averages. Just as in the method of averages we deal with summations of partial sets of observations, so we shall here deal with summations for partial sets of tests or points. In fact, we shall define the x-axis so that it passes through the origin and through the center of gravity of a partial set of points. The y-axis will be so determined that the xy-plane contains the x-axis and the center of gravity of a second set of points. This procedure can be continued for any number of dimensions or factors. The number of dimensions of the sphere is the same as the number of general factors that are postulated. This procedure will now be described in more detail.

In locating the coördinate axes there are several criteria and several quantitative tests that may be applied. These criteria will be considered in a separate paper, and we shall here describe only one such criterion by which the axes may be located. Assume that you have before you the sphere with a point designated on its surface for each test or variable in the correlation table. Our first problem is to locate the x-axis through this sphere. The projection of a point on this axis will be the loading of the first factor in that test. Suppose that we arbitrarily pass the x-axis through test a. Then this test will have loading of unity for the first factor and zero for all of the other factors. The projection of test b on this axis will then be the loading of the first factor in

test *b*. In dealing with actual data we shall always have a residual composed of minor general factors that are ignored and specific factors peculiar to each test or variable. For this reason we want to account for as much as possible of the correlations in terms of the smallest possible number of general independent factors. We therefore want to pass the *x*-axis through this sphere so as to maximize the projections of all the points on it.

We first find that one test which has the highest average correlation with all the other tests, disregarding sign. We disregard sign because it does not matter here whether the projection is positive or negative. The sign is arbitrary and either projection serves to determine the correlation. The sum of each column in the correlation table, disregarding sign, is recorded. Let the test with the highest sum be designated test *a* and let the sum be designated $\Sigma|r_{ak}|$. This test is evidently more nearly like the rest of the tests in the series than any other single test. The average distance from it to all the other tests is, in general, smaller than the corresponding average separation of each other test from all the rest. We have found now the most representative single test in the battery. Of course we could now simply pass the *x*-axis through this test and thereby make sure that we have nearly maximized the projections of all the tests on this axis. But we do not want to define any of the axes by any single test or variable. We therefore make a list of all the tests that correlate positively with test *a*. These tests will all lie in a hemisphere with test *a* at its pole. This is clear because all positive correlations correspond to angles less than 90 degrees, and all correlations that are negative correspond to angles greater than 90 degrees. We can now be fairly sure, but not absolutely so, that this partial group of tests have something in common that is a conspicuous and important general factor in the whole set of tests. We shall define the *x*-axis so that it passes through the center of the sphere, of course, and also through the center of gravity of all the tests in the hemisphere with *a* at its pole.

Let us designate all the tests which correlate positively

with test a by the general notation s. The notation s refers then to each of the tests in this partial group, taken in succession. The correlation of test a with each of the tests in this sub-group s may then be written as

$$r_{as} = a_1 s_1 + a_2 s_2 + a_3 s_3. \tag{6}$$

Summing, we have

$$\Sigma r_{as} = a_1 \Sigma s_1 + a_2 \Sigma s_2 + a_3 \Sigma s_3, \tag{7}$$

in which Σs_1 is the sum of the projections of the points in the set s on the first axis, Σs_2 is the sum of the projections of the points in this set on the second or y-axis, Σs_3 is the sum of the projections on the third or z-axis.

The center of gravity of the points in the set s is a point inside the sphere and since we have taken a set of tests in a single hemisphere we are certain that this center of gravity will not lie at or near the center of the sphere. If that should happen our subsequent determinations would be very inaccurate. The coördinates of the center of gravity of the set of points s will evidently be

$$\frac{\Sigma s_1}{N_s}, \quad \frac{\Sigma s_2}{N_s}, \quad \frac{\Sigma s_3}{N_s}$$

with reference to the three axes that are still to be chosen. Now let us pass the first axis, the x-axis, through the center of gravity of the set of points s. But if this center of gravity lies on the x-axis, then it is clear that the y- and z-coördinates must be zero so that

$$\Sigma s_2 = \Sigma s_3 = 0.$$

The correlation between the test a and any one of the other tests in the set s can then be simplified because the last two terms in equation (7) vanish. The expression for this correlation becomes then

$$\Sigma r_{as} = a_1 \Sigma s_1 \tag{8}$$

or, rewriting this explicitly for the loading a_1, we have

$$a_1 = \frac{\Sigma r_{as}}{\Sigma s_1} \tag{9}$$

By analogy with equation (5) we can write the following corresponding equation for the set s,

$$(\Sigma s_1)^2 + (\Sigma s_2)^2 + (\Sigma s_3)^2 = \Sigma r_{ss} \tag{10}$$

in which Σr_{ss} is the sum of all the correlation coefficients in the full table of the s tests, including self correlations taken as unity, and recording each coefficient twice in the table since it is symmetrical. But since Σs_2 and Σs_3 are zero by the location of our x-axis, we have the simpler relation

$$(\Sigma s_1)^2 = \Sigma r_{ss} \tag{11}$$

or

$$\Sigma s_1 = \sqrt{\Sigma r_{ss}}$$

and from this we know the loading of the first general factor in each test as follows:

$$\begin{aligned} a_1 &= \frac{\Sigma r_{as}}{\sqrt{\Sigma r_{ss}}}, \\ b_1 &= \frac{\Sigma r_{bs}}{\sqrt{\Sigma r_{ss}}}, \\ &\cdot \quad \cdot \quad \cdot \\ w_1 &= \frac{\Sigma r_{ws}}{\sqrt{\Sigma r_{ss}}}. \end{aligned} \tag{12}$$

It should be noted that these relations are valid even though the test b, for example, is not a member of the particular set of points s. Hence the above simple relation enables us to determine the loading of the first general factor in each of the w tests.

We now want to determine the loading of the second general factor in each of the tests. Imagine again the sphere with the w points allocated to its surface. We have now passed the x-axis through this sphere. The y-axis must of course be at right angles to the x-axis because we assume that the general factors are uncorrelated. The angle between the first and second axes must therefore be 90 degrees. If we revolve the sphere about the x-axis with test a somewhere in the vicinity of the pole, it is clear that the y-axis which shall represent the second general factor will pierce the surface of

the sphere somewhere in the equator. We shall now select a pivot test for the second factor which shall serve the same function in locating the second factor that test *a* served in locating the first factor. This second test will clearly lie in an equatorial band and it will have a low correlation with test *a*. We therefore tabulate separately all the tests that have a low correlation with test *a*. In doing so we must decide how wide an equatorial band is to be included. The band should be wide enough to include all tests that are essentially different from test *a*, but the band should not be so wide as to include those tests which are heavily loaded with the first factor which is already represented by the *x*-axis. The width of the equatorial band selected will depend also in part on the number of tests in the whole series of intercorrelations. Let us decide to use an equatorial band not wider than 30 degrees on either side of the equator. An angle of this size has a cosine of .50 so we make a list of all tests that correlate less than .50, either positive or negative, with test *a*.[2]

In order to find the most representative test in the equatorial band, we inspect again the summations $\Sigma|r_{ak}|$, $\Sigma|r_{bk}|$, $\Sigma|r_{ck}|$, $\cdots$ $\Sigma|r_{wk}|$, disregarding sign. We find the test *b* in this list which has the highest sum of its correlations with the other tests. In the equatorial band this test is the most representative of all the tests in the whole series *w*. By selecting this one as our second pivot test we will, in general, but not necessarily, maximize the projections of all the tests on the second axis. Let this second pivot test, so selected, be test *b*.

We now tabulate, as before, all those tests in the whole series *w* which correlate positively with test *b*. Let this second sub-group of tests be designated by the notation *t*. We have now a set of tests *t* which lie in a hemisphere with test *b* as its pole. Consider the center of gravity of this set of points *t*. It will be a point inside the sphere but it will

[2] In case all the intercorrelations in the initial table are positive it is clear that all the points lie in one quadrant or octant of the surface and hence the selection of the subgroups *s*, *t*, *u*, must be adapted to the distribution of the initial coefficients as to positive and negative values.

certainly not coincide with the center of the sphere. That is to be avoided in the interest of accuracy of subsequent determinations. Now we shall locate the y-axis so that the xy-plane contains the x-axis and also the center of gravity of the set of points t. By so doing we make the z-coördinate of the center of gravity of the set t vanish. By this procedure in locating the successive axes we reduce the number of unknowns so that the loadings may be readily determined.

The correlation between test a and any one of the tests in the set t may be written

$$r_{at} = a_1t_1 + a_2t_2 + a_3t_3. \tag{13}$$

Summing for all the tests in the set t, as before, we have

$$\Sigma r_{at} = a_1\Sigma t_1 + a_2\Sigma t_2 + a_3\Sigma t_3. \tag{14}$$

Here the values of Σt_1, Σt_2, Σt_3, are the sums of the projections of the points in the set t on each of the three coördinate axes.

The coördinates for the center of gravity of the set of points t are evidently

$$\frac{\Sigma t_1}{N_t}, \quad \frac{\Sigma t_2}{N_t}, \quad \frac{\Sigma t_3}{N_t},$$

but the z-coördinate vanishes by so locating the y-axis that the xy-plane contains the center of gravity for the set of points t. Therefore

$$\Sigma t_3 = 0$$

and hence

$$\Sigma r_{at} = a_1\Sigma t_1 + a_2\Sigma t_2 \tag{15}$$

Since all the x-coördinates are now known, so is also Σt_1 for the set t. Therefore we can solve for a_2 in the above equation in the form

$$a_2 = \frac{\Sigma r_{at} - a_1\Sigma t_1}{\Sigma t_2}. \tag{16}$$

The summation Σt_2 is known from the relation

$$\Sigma r_{tt} = (\Sigma t_1)^2 + (\Sigma t_2)^2, \tag{17}$$

which can be written by analogy with equation (5). This equation enables us to determine the loadings of the second factor in all of the tests.

The loadings of the second factor in the remaining tests are determined by equations analogous to (16), namely

$$b_2 = \frac{\Sigma r_{bt} - b_1 \Sigma t_1}{\Sigma t_2}, \qquad (16)$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot$$

$$w_2 = \frac{\Sigma r_{wt} - w_1 \Sigma t_1}{\Sigma t_2}.$$

When the loadings of the first two factors in each of the tests have been determined it would be possible to determine the loading of the third factor from the relations

$$a_1^2 + a_2^2 + a_3^2 = 1,$$
$$b_1^2 + b_2^2 + b_3^2 = 1,$$
$$c_1^2 + c_2^2 + c_3^2 = 1,$$

and so on provided that we were certain that three factors were sufficient to determine the correlation coefficients. However, we must assume that in any ordinary situation there are residuals composed of additional general factors of minor importance, perhaps, and specific factors peculiar to each of the tests. On this account we shall not make use of the above equation for determining the loadings of the third factor. These loadings will be determined in a manner similar to that used for the previous loadings.

We make a list of all the tests that correlate between + .50 and − .50 with *both* tests *a* and *b*. In this manner we shall be sure that we have a list of tests represented by points which are in the general vicinity of a right angle from both *a* and *b*. We now leave the restricted three dimensional sphere and proceed by analogy into higher dimensions. We select that one test in this list which has the highest sum Σr_{kk}, disregarding sign. Let this test be test *c*. This will be the pivot test for the sub-group of tests which are to be used for determining the third general factor.

After having found the pivot test for the third general factor we list all of the tests that have positive correlations with test *c*. Let this sub-group be designated *u*. These will all lie in a hemisphere with test *c* at its pole. We shall

so locate the third axis or factor that the center of gravity of the set of points u has finite values for the first three coördinates, and so that the higher numbered coördinates of this center of gravity will vanish. This is merely carrying out the same procedure as before. The correlation between test a and any one of the tests in set u will then be

$$r_{au} = a_1u_1 + a_2u_2 + a_3u_3. \tag{17a}$$

Summing we have

$$\Sigma r_{au} = a_1\Sigma u_1 + a_2\Sigma u_2 + a_3\Sigma u_3. \tag{18}$$

This equation can be solved for a_3 since all the other values are known. The values of Σu_1 and Σu_2 are known because we have the loadings of the first and second factors in each of the tests and we have listed the tests that belong in the set u. The summation Σr_{au} is known directly from the given correlation coefficients. The value of Σu_3 can be obtained from the equation

$$\Sigma r_{uu} = (\Sigma u_1)^2 + (\Sigma u_2)^2 + (\Sigma u_3)^2, \tag{19}$$

which is written by analogy with equation (5). Hence

$$a_3 = \frac{\Sigma r_{au} - a_1\Sigma u_1 - a_2\Sigma u_2}{\Sigma u_3}, \tag{20}$$

$$b_3 = \frac{\Sigma r_{bu} - b_1\Sigma u_1 - b_2\Sigma u_3}{\Sigma u_3},$$

$$\cdots\cdots\cdots\cdots$$

$$w_3 = \frac{\Sigma r_{wu} - w_1\Sigma u_1 - w_2\Sigma u_3}{\Sigma u_3}.$$

In the same manner we may extend the procedure to any number of factors that may be necessary to account for a given table of intercorrelations. However, it is not advisable to carry this procedure so far as to determine the correlations within the errors of measurement because in that case the last factors are likely to be merely the loadings that are necessary to adjust for chance errors. Our purpose is to discover only those principal factors that are truly operative in producing the correlation coefficients. Hence this procedure should be carried out far enough to lock the coefficients

with discrepancies somewhat greater than the chance errors in the given coefficients.

Finally, when the loadings have been determined for each test, one should calculate all of the coefficients by equations (2). These can then be compared with the given experimental coefficients. The discrepancies $d = r_e - r_c$ in which r_c = calculated r, and r_e = experimental or observed r, should then be calculated. A frequency distribution of these discrepancies will indicate fairly well how closely the factors account for the given coefficients. In no case should one expect the standard deviation of this distribution to be the same as the average standard error of the given coefficients. The standard deviation of the discrepancies will be the larger because it may be generally assumed that the limited number of general factors that are postulated do not include the minor group factors and specific factors that together will make the residuals greater than those expected by chance errors alone.

In order to facilitate the application of the procedure we shall list here the successive steps together with the fundamental equations that are necessary in solving for the successive values.

The following outline has been extended to include four factors in order to illustrate the method.

(1) Prepare a full table of intercorrelations for the w tests. Assume that all self correlations are unity and record them so. The full table will be symmetrical in that every coefficient will occur in two cells. Thus the coefficient r_{ab} will occur in column a opposite b and also in column b opposite a. Record the sums $\Sigma|r_{ak}|$, $\Sigma|r_{bk}|$, $\Sigma|r_{ck}|$, etc., for all columns, disregarding sign. The subscript k refers to each of the w tests in the whole series taken in succession.

(2) Find that test a which has the highest sum for its column, namely $\Sigma|r_{ak}|$.

(3) Make a list of all the tests that correlate positively with test a. This is done by listing all tests with positive correlations in column a. Let this sub-group of tests

be designated s. The sum Σr_{ss} is the *algebraic* sum of all the coefficients in a full table of the tests in group s. Then

(4) $$(\Sigma s_1) = \sqrt{\Sigma r_{ss}}$$

(5) $$a_1 = \frac{\Sigma r_{as}}{\sqrt{\Sigma r_{ss}}}$$

$$b_1 = \frac{\Sigma r_{bs}}{\sqrt{\Sigma r_{ss}}}$$

. . .

$$w_1 = \frac{\Sigma r_{ws}}{\sqrt{\Sigma r_{ss}}}$$

The loading of the first factor in every test is now determined.

(5a) Determine the algebraic sum Σs_1 from the list of first factor loadings and see that it agrees with the value found in step 4.

(6) Make a list of all tests that correlate with test a within the range $\pm .50$. These tests are in general different from test a. Select that test b in this list which has the highest sum in its column, namely $\Sigma|r_{bk}|$, disregarding sign.

(7) Now make a list of all the tests that correlate positively with test b and let this sub-group be designated t.

(8) Determine (Σt_1). This can be done since we know the first factor loading in each test and we have a list of the tests in group t. Also determine Σr_{tt} for the sub-group t.

(9) Determine

$$\Sigma t_2 = \sqrt{\Sigma r_{tt} - (\Sigma t_1)^2}.$$

(10) Determine

$$a_2 = \frac{\Sigma r_{at} - a_1 \Sigma t_1}{\Sigma t_2},$$

$$b_2 = \frac{\Sigma r_{bt} - b_1 \Sigma t_1}{\Sigma t_2},$$

. . . .

$$w_2 = \frac{\Sigma r_{wt} - w_1 \Sigma t_1}{\Sigma t_2}.$$

(10*a*) Determine the sum Σt_2 from the list of second factor loadings and make sure that it agrees with value found in step 9.

(11) Make a list of all tests that correlate low, say within the range $\pm .50$, with *both* tests *a* and *b*. If there are no such tests, then you need no additional general factors and this procedure need not be carried any further. Select that one test *c* in this restricted list that has the highest sum $\Sigma|r_{ck}|$, disregarding sign.

(12) Make a list of all tests that correlate positively with test *c* and let this sub-group be designated *u*.

(13) Determine Σu_1 and Σu_2. This can be done since we have the first and second factor loadings for each test and we have a list of all tests in the sub-group *u*. Also determine Σr_{uu} which is the algebraic sum of all coefficients in the full table for group *u*.

(14) Determine

$$\Sigma u_3 = \sqrt{\Sigma r_{uu} - (\Sigma u_1)^2 - (\Sigma u_2)^2}.$$

(15) Determine

$$a_3 = \frac{\Sigma r_{au} - a_1\Sigma u_1 - a_2\Sigma u_2}{\Sigma u_3},$$

$$b_3 = \frac{\Sigma r_{bu} - b_1\Sigma u_1 - b_2\Sigma u_2}{\Sigma u_3},$$

. . . . .

$$w_3 = \frac{\Sigma r_{wu} - w_1\Sigma u_1 - w_2\Sigma u_2}{\Sigma u_3}.$$

(15*a*) Determine the sum Σu_3 from the list of third factor loadings and make sure that it agrees with the value found in step 14.

(16) Make a list of all tests that correlate low, say within the range $\pm .50$, with the tests *a*, *b*, and *c*. If there are no such tests, then you need no additional factors. If there are several such tests, continue as follows. Select that one test *d* in this restricted list that has the highest sum $\Sigma|r_{dk}|$, disregarding sign.

(17) Make a list of all tests that correlate positively with test *d* and let this sub-group be designated *v*.

(18) Determine Σv_1, Σv_2, and Σv_3. This can be done since we have the first, second, and third factor loadings in each of the w tests and we have a list of the tests in sub-group v. Also determine the algebraic sum Σr_{vv}.

(19) Determine

$$\Sigma v_4 = \sqrt{\Sigma r_{vv} - (\Sigma v_1)^2 - (\Sigma v_2)^2 - (\Sigma v_3)^2}.$$

(20) Determine

$$a_4 = \frac{\Sigma r_{av} - a_1\Sigma v_1 - a_2\Sigma v_2 - a_3\Sigma v_3}{\Sigma v_4},$$

$$b_4 = \frac{\Sigma r_{bv} - b_1\Sigma v_1 - b_2\Sigma v_2 - b_3\Sigma v_3}{\Sigma v_4}$$

. . . . . .

$$w_4 = \frac{\Sigma r_{wv} - w_1\Sigma v_1 - w_2\Sigma v_2 - w_3\Sigma v_3}{\Sigma v_4}.$$

It is evident that this procedure can be extended in cycles of five steps for each new factor. Thus a new factor is started in steps 1, 6, 11, 16, 21, and new factors would appear in steps 26, 31, 36, and so on, if the process were continued. The process should be continued until no new tests appear in the lists prepared in each of the steps just enumerated.

In this manner one can make some rational estimate of the number of factors that will be needed to account for any given table of coefficients. Note that this estimate can be made by preparing the lists of tests called for in steps 6, 11, 16, 21, 26, 31, and so on until the list vanishes, before any calculations are started. The number of factors can thus be estimated merely by inspection of the coefficients and without any calculating whatever. This enables one to lay out in advance the data sheets for a postulated number of general and independent factors.

As stated at the outset there are several other criteria that may be applied in selecting the general factors or coördinate axes which are of importance in special cases where unusual clustering of the tests or variables may be suspected. Some special methods of locating the coördinate axes will be considered in a separate paper.

We have described a method of multiple factor analysis

by which it is possible to ascertain how many general, independent, and uncorrelated factors it is necessary to postulate in order to account for a whole table of intercorrelations. The method is free from any limitations about group factors. Objective methods have been described for locating the general factors and it is probable that, except in unusual distributions of the tests, these methods will prove adequate.

After the factor loadings have been determined for all of the tests or variables, it is of considerable interest to describe or name the general factors that have been isolated. This can be done best by noting which tests or variables have a relatively high positive loading with the first factor and which have a low or negative loading with this factor. By this inspection it is possible to name the first factor although the statistical procedures do not of course concern these matters of describing or naming the factors. In a similar manner one might inspect the second factor loadings to ascertain which tests have positive loadings and which have negative loadings with this factor. A name might then be found for this factor and so on.

Finally, it may be of interest to assign the standard score in each of the factors to each of the individual subjects. If each factor represents an ability of some kind, then it would be of interest to be able to assign to each person a standard score in each of the factors or abilities that have been isolated. In the following list of equations the only unknown factors are x_1, x_2, x_3, and x_4. These standard scores in the four abilities for any one person may be determined by solving the following observation equations for the four unknown standard scores by the method of least squares or by the method of averages. This procedure is laborious but it is at least possible to solve the problem of individual standard scores in the several independent abilities.

$$
\begin{aligned}
S_a &= a_1x_1 + a_2x_2 + a_3x_3 + a_4x_4, \\
S_b &= b_1x_1 + b_2x_2 + b_3x_3 + b_4x_4, \\
S_c &= c_1x_1 + c_2x_2 + c_3x_3 + c_4x_4, \\
&\cdots\cdots\cdots\cdots \\
S_w &= w_1x_1 + w_2x_2 + w_3x_3 + w_4x_4.
\end{aligned}
$$

It is probable that these methods of multiple factor analysis will be useful in discovering how many factors underlie a given table of correlation coefficients and in discovering their general nature. Several applications of the method to different types of correlation problems are under way and these will be reported in subsequent papers.

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MENTAL ACTIVITY AND THE MUSCULAR PROCESSES

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Some causal relationship between mental activity and the muscular processes has been urged through various periods of changing psychologic thought. The consequent hypotheses which have been advanced are likely to leave the critical reader unconvinced. Nevertheless, both casual and experimental observations point to an interdependence of some sort. New facts call for new analyses; and the excuse for the present paper is the pertinency of certain results of recent research in muscular tension.

The majority of attempts to relate mental and muscular activity have been speculative schemata rather than working hypotheses capable of experimental verification. Their common tendency has been to discuss the relation on the basis of our philosophical heritage (interactionism, parallelism, epiphenomenalism) or from some more empirical but equally ineffectual point of view. As I read over this mass of literature, I am impressed not so much with the fallaciousness of the assumptions made as with their general futility. Certain factual difficulties are immediately apparent in many of the so-called 'motor' accounts of mental functioning; but a far greater number are quite inaccessible to experimental attack and criticism. Confusion seems to have resulted both from the attempt to retain 'consciousness' as one term in the functional relationship and from the endeavor to apply the outworn doctrine of circular reflex action.

It will be my thesis that the relevant doctrine is best understood as dealing with the *central facilitation and regulation of cortical operations*. According to this thesis, the facts of experience are to be regarded only as *indicators* of central

function and their correlation with variations in muscular activity to be rendered intelligible solely by reference to principles of central nervous operation. Before this position may be developed further it is advisable briefly to consider the adequacy of existent theory to the relationship it seeks to explain.

II

The presence of 'expressive movements' and 'kinæsthesis' in temporal contiguity with experiential events has had various treatments in 'motor' theory. All hypotheses, however, have one essential factor in common; in venturing to achieve a neurological context, they revert to the time-worn concept of the completed reflex arc. As usually applied to the problem of cerebral functioning, this doctrine conceives the nervous system as a mere conductor, incapable of energizing itself. Expressive movements and kinæsthesis are considered as events of the same order and are cited as valid evidence that the neural 'switchboard' is engaged in rapidly transmitting the effects of stimulation into 'overt' or 'tentative' or 'implicit' responses.

So thoroughly ingrained is this general notion that one might assume the relationship under discussion incapable of other explanation. Theorists argue among themselves as to whether the mental processes function only when the laryngeal muscles are active (Watson, **30**), and whether they are associated with striped or smooth muscle (Langfeld, **22**), or both (Dashiell, **4**). Some say that consciousness is clearest when the motor elements discharge most freely (Münsterburg, **27**), while others would have it that checking of the motor activity makes conscious processes more vivid (Montague, **26**); or the two views may be combined by making 'consciousness' the consequence of a particular *ratio* of excitation to inhibition in the activated motor elements [1] (Washburn, **31**). In spite

[1] Abel (1) has recently criticised Washburn's theory upon the grounds that without the brain as a 'deus ex machina' the theory is sieve-like. What, she asks, is the elusive factor which enables the brain to turn a trick which cannot be done at the spinal level (where there are numerous excitation-inhibition ratios)? This objection would not hold, of course, if one were committed to the assumption that all neural processes have elementary psychic character. I take it, however, that supposititious cord-

of all these divergences, there would still be general agreement that somehow or other afferent stimulation must eventuate in some effector 'response' before consciousness of the afferent stimulation can occur.[2] Langfeld's recent statement is typical in this respect; "It is not justifiable to single out a single segment of the neuro-muscular mechanism as being conscious . . . the entire system . . . must be considered as a whole" (**22**).

Even if we agree with this statement, how does it aid our understanding of mental dynamics—especially when *cortical processes* really figure so prominently in the scheme? Must we assume that these processes are active only when afferent impulses to them pass out as efferent impulses? Or may not the muscular processes have some function other than the mere completion of selected nervous curcuits? These questions can be answered only by considering the alleged adequacy of the reflex theory to the description of cerebral action as well as the advisability for alternative theorizing.

It is generally apparent to the oriented reader that the processes of neural integration are much more complicated than the reflex theory conceives them to be. The balance of evidence favors the view that the nervous system acts as a total integrated unit rather than as a mere aggregation of isolated pathways; and the principles of irradiation, altered excitability, induction, stratification, etc.—of which more anon—are more or less foreign to this paradigm of cortical action. In a pointed criticism of such inadequacies, Lashley (**24**) has recently concluded that "there are centrally maintained processes, which if they become intense, *may* irradiate to motor centers and produce expressive movements, implicit speech and the like." So far, I am in agreement with this alternative explanation. It is indeed highly probable that

consciousness (Pflüger) is quite beyond the realm of experimental attack; and if we are going to assign an element of the 'mental' to the 'organic memory' of even colloids (Sémon, Rignano), that term certainly loses the meaning it holds for our own phenomenal experience.

[2] The reader's attention is called, in this connection, to a very suggestive though not entirely decisive experiment which showed that a 'conditioned salivary reflex' could be acquired in the absence of the salivary (effector) response (*Amer. J. Physiol.*, 1930, **94**, 553–556).

much overt movement is an incidental consequence rather than a cause of cortical action. On the other hand, I cannot continue with Lashley that "peripheral activities are not an essential condition for the maintenance of the central processes." It seems to me that certain muscular processes, particularly those of tonic contraction, function in just such maintenance. Misconception and a desire for over-simplicity has resulted in the incorporation of these processes (together with the 'expressive movements') in the reflex notion of cerebral activity; their importance has been neglected rather than specifically denied by Lashley. I conjecture that a theory of central facilitation by way of proprioceptive stimulation is in accord with the presumptive importance of these processes and with current neurological trends. I further believe that the critical examination of the correlation of mental activity with these processes, now made possible by refined techniques, reveals substantial support for such a theory.

III

We have now to review the specific evidence from which our theory logically develops. The study of the rôle of muscular tension in cortically integrated behavior has progressed along two lines. In the first class of researches, the problem has been to vary some phase of higher behavior and observe its effects upon the tonus accompaniments. Other researches have had as their problem to observe the effects of variations in muscular tension upon behavior. Only the major results of both types of approach may be summarized here.

1. The first investigation of the tonus accompaniments of cerebration was made when the knee jerk was the only medium of study. A number of subsequent investigators (summarized in 3) confirmed the reported increase in muscular tension during mental 'effort.' So far as I can make out, only one of these writers [3] holds that such responses are accessory to the

[3] Golla (11) tends to see in his work phenomena which are *atavistic* and not to be confused with tensions of voluntary origin (which he concedes may be facilitative). Aside from the grave difficulties attending the attempted separation of the 'reflexive' and 'voluntary' phases of behavior, one may question whether Golla's research actually

efficiency of the psychological performance. My own conviction of a facilitative effect was first greatly strengthened by certain diurnal observations made on myself. I found that the amount of tension, as measured in my right quadriceps at half hour intervals, closely paralleled the efficiency of my cerebration throughout the day. Tonus was lowest at waking, rose rapidly during the early morning, declined in the afternoon and reached a new peak in the evening. Tonus was highest on days following serious sleep loss and there was generally recorded a higher level for night work that equalled the efficiency of my morning labor. We know further that tonus is noticeably reduced in sleep and that the attentive consciousness is accompanied by an increase in muscle tone.

Another indirect line of evidence for the presence of muscular tension as a condition of efficient cerebration is the comparison of output immediately upon waking with that later in the day. A typical experiment is that of Johnson and Swan (**18**) which showed that work done just before retiring was 6.5 percent superior to work done immediately on waking. Their suggestion that muscular tension 'supplies the major portion of the nervous current utilized in effecting response' is reflected elsewhere in the experimental literature.[4]

2. The studies of experimentally induced tension show even more clearly that muscular activity is a condition of effective waking behavior. The most impressive results with human reactors were obtained by Miller (**25**), whose subjects responded to electric shocks during extreme relaxation. Compared to control experiments, such muscular inactivity reduced the extent of finger reaction and increased the reaction time. The subjects also reported *an apparent diminution in the intensity of the stimulus.* The work on progressive relaxation (Jacobson, **14**) has brought out the fact that all

showed no facilitative effect. Simply because these effects were not as obvious as those obtained by some of his contemporaries, he does not need to conclude that the majority of tonus accompaniments of mental work fulfill no vital function.

[4] Moore *et al* (*Amer. J. Physiol.*, 1922, **59**, 471) found some evidence of a direct relationship between duration of sleep and the amount of dynamometric work done. Haas (*Psychol. Arb.*, 1923, **8**, 228) found his calculating ability to be inversely related to his depth of sleep. (It will be recalled that Haas' sleep was alleged deepest shortly after retiring.)

mental processes may be 'relaxed' away. It will be recalled that muscular relaxation also abolished the knee jerk (Jacobson and Carlson, **15**). In all these researches we have examples of reduced cortical and reflex irritability as the simple result of relaxation. On the other side of the picture, Bills (**3**) has shown that the rate of learning various tasks is facilitated by experimentally induced tension (*e.g.*, produced by gripping a dynamometer).

Now the theoretical implications of such results are generally vague and unclear. We find various interpretations applied by contemporary writers. I need but cite a few of the more available references. "It is through them [the afferent organs excited by muscular tension] that much of the nervous current is generated which is necessary to normal adjustment" (Johnson, **16**). "Are we not forced to conceive of a special form of nervous energy in the manifestation of muscle tone and posture" (Myers, **28**). "At any given time, a certain set of stimuli produce neuro-muscular tensions by means of collaterals to the sympathetic nervous system [5] and through short routes back to the external muscles of the body. These tensions determine the high potential areas of the brain" (Wheeler, **32**).

It is more or less apparent from the above excerpts that each of these authors is talking about the *facilitative effect* of tonic contraction. It appears further that while no author is entirely satisfied with the conventional telephonic switchboard analogy to this effect, there is no clear notion of what might be substituted. Each is attempting to explain how muscle tonus may 'energize' the higher neural processes; yet the more elaborate speculations bring discredit to the relationship already discussed and the conservative accounts are too nebulous and vague to be very helpful. I conjecture that a general theory of neural 'vigilance' (Head, **12**) forms the most adequate basis for understanding the muscular reinforcement of cortical operations. This theory must be appreciated in its

[5] This theory applies configural principles to Dewey's concept of the reflex arc. We realize, however, that the experimental support of the alleged sympathetic innervation of tonus is dubious. (*Cf.* the writer's first article of this series, *Amer. J. Psychol.*, 1930, **42**, 581–592; also Forbes, *Arch. Neur. & Psychiat.*, 1929, **22**, 247–264.)

broader aspects before the rôle played by the musculature can be appreciated.

IV

The excitability of neural centers was early conceived to vary with their anatomical locus and physiological function (Hughlings-Jackson). This principle of differential sensitivity is obviously basic to the concept of protopathic and epicritic stratification of behavior. The nervous system reacts, according to Head, with varying degrees of physiological efficiency, dependent upon its vital condition or 'vigilance.' "The same neural centers will give different responses to identical stimuli dependent upon their state of vitality. Consciousness stands in the same relation to the vigilance of the higher centers as adaptive and purposive reflexes to that of those of lower rank in the neural hierarchy. When vigilance is raised, a response to stimulation is not only obtained more easily, but shows signs of belonging to a higher order than it is low."

What, we may ask, are the factors responsible for the degree of 'vigilance' in a neural center? "Vigilance is diminished not only by structural changes in the central nervous system, but also by toxic influences (such as chloroform and sepsis) or *anything else which tends to lower physiological capacity*." It seems to me that the absence of proprioceptive stimulation of cortical centers is a very important one of these other conditions. I further believe that its inclusion becomes necessary in order for the doctrine of 'vigilance' to be complete. Certain it is that the lowered vitality of centers by humoral mechanisms alone is insufficient to account for the phenomenon of sleep.[6] In the explanation of this and many other variations in behavior, it is helpful to enlarge the concept of 'vigilance' to include the cellular effects not only of substances carried by the blood but also of neural excitation irradiating from other centers.

There are, presumptively, a number of internal processes which act conjointly in the common task of initiating, sus-

[6] We now realize that sleep is more than the result of fatigue toxins or anemia in brain centers; furthermore, it may come more frequently from a restriction of muscular activity rather than from over-exertion.

taining and reinforcing the action of higher centers. In the remainder of this paper, however, I shall confine my discussion to the processes of tonic contraction, particularly because their facilitative action seems the most obvious and open to experiment at the present time.

V

To explain the exact means by which contraction of the musculature facilitates cortical activity we need resort neither to the outworn doctrine of the circular reflexes nor to some new and utterly untried hypothesis. What we have to deal with is something analogous to *summation*. Pike (**29**) has long held the view that any neural center reacts to a summation of stimuli.[6a] A legitimate extension of this doctrine would make the activity of the central processes involved in sensation dependent upon impulses emanating from some exteroceptive source *plus* impulses of proprioceptive origin. The addition of such 'energies' would then be said to result in the stimulation of the cortical cells involved. We shall see that this rough description is also relevant to the problem of how the central synergies of memory and thought are marshalled and sustained.

Summation is, of course, far too simple a principle to account for the facilitative effect of proprioceptive stimulation upon cortical neurones. It is less neurologically naïve to conceive such action as *altering their thresholds of irritability*. As a matter of fact, research on animals indicates that this may actually take place.[7] That the facilitative effect is generalized (*i.e.*, not necessarily confined to specially favored neural channels) is an inference based partially on the knowledge of the cortical effects of electrical stimulation and the

[6a] Though Pike is usually credited with this notion, it is not original with him. Huphlings-Jackson, Lewes, Wundt and Sherrington hold somewhat similar views.

[7] Space permits only a few samples: Light muscular work increased the conditioned salivary reaction of dogs (Bykow, *C. R. S. Biol.*, 1927, **97**, 1398). This work is not crucial due to the fact that no control series was run to determine the effect of the muscular work upon the 'unconditioned reflex.' More significant is the work of Cordot *et al.* (*C. R. S. Biol.*, 1927, **97**, 698) where muscular contraction was shown to lower the chronaxie of cortical areas of experimental preparations. See also the two articles of A. Rizzolo in the same volume of that journal (pp. 1608–1611, 1607–1608).

fact that a diversified number of reactions may be equally facilitated by approximately the same amount of muscular tension. We shall see later that there is nothing to prevent the existence of specific patterns of excitation and inhibition upon a background of more general facilitation.

It would not help the present exposition to detail how threshold-alteration may be produced in cellular units. The reader specifically interested in these more technical phases will find it best to go to the sources.[8] We may proceed at once, therefore, to state the relationship between mental activity and the muscular processes in neurological terms, as follows: *The degree of completeness of higher neural integration (called mental) will vary with the amount of afferent stimulation of the cortex set up and maintained in great part by the processes of muscular tension. The excitation irradiated by these processes acts to lower the thresholds of irritability in the higher (cortical) centers. The increased excitability of these centers by such means is an essential condition for their reaction to specific patterns of external and internal stimulation of normal intensity during waking.*

Very few will deny that muscular contraction sometimes facilitates mental activity. The chief criticisms of the above formulation are likely to be that its scope is unnecessary and unwarranted. I am convinced that neither of these criticisms hold.

It may be argued that the relationship under discussion may be more simply 'explained.' Our tradition includes the fact that many different activities facilitate and inhibit each other. Of what value is it, then, to discuss hypothetical neural mechanisms upon the basis of insufficient knowledge? If for no better reason, I should say because the terms 'facilita-

[8] Excellent reviews of the work on chronaxie (Fredericq, *Physiol. Rev.*, 1928, 8, 501) and surface film tensions (Lillie, *Physiol. Rev.*, 1922, 2, 1) are readily available. H. M. Johnson has recently (*J. Compar. Psychol.*, 1927, 7, 187) postulated a mechanism by which modification of the 'film-frequency' of a neurone might be produced by afferent discharges from both striped and smooth muscle. While I am in general agreement with this detailed analysis of muscular reinforcement, I do feel that it is neurologically less obscure if we refer the 'organization' of proprioceptive and exteroceptive stimulation to the cerebral cortex—at least for conscious behavior. This view Johnson apparently discards as 'now moribund.'

tion' and 'inhibition,' which are purely description expressions, have been so abused as *explanatory principles* that they have become mere shibboleths to loose thinking. Their common usage does not conceal their inadequacies any more than they 'explain' neural action. Specific formulations, no matter how sketchy, are the only ways at hand for bringing meaning to such terms.

Suppose that two fire bells of equal intensity sounded during the night. I heard the first, but not the second. Exactly *what was the facilitative effect* which made the first stimulus adequate for experience, the second—which occured during sleep—not. In an attempt to answer such questions as these, psychologists—and physiologists as well—have resorted to the dubious postulation of special facilitative and inhibitory 'centers' in the cortex. As frequently happens, the obvious answer may prove the most adequate. The laymen, describing my behavior during both periods of bell-ringing will report that during the first I was alert and tense; later he could tell I was asleep by my relaxed posture. This simple observation suggests a general differentia which is in accord with our hypothesis and which, by the way, is capable of a high degree of experimental investigation.

One objection to this line of argument is immediately obvious: "How can we be sure that a relaxed man is not awake?" I would prefer the question to be put, "How can we be sure the awake man is quite relaxed?" During a prolonged period of insomnia, I frequently feigned sleep so as not to disturb my wife; yet I felt the muscles of my legs and arms grow more and more tense with time. One must be careful to avoid the fallacious assumption that muscular work will reveal itself overtly. Even James [9] apparently made this slip in discussing the insignificant exertion and great effort of the man about to be fired o.

A far more important objection would be that the facilitative effect of muscular contraction is not a universal necessity for efficient mental work. There is some support for this view. Casual evidence would lead us to expect that the most

[9] W. James, Psychology, Briefer Course, New York, Holt, 1920, 450.

efficient brain-worker uses a minimum of muscular tension. The questioning of eminent men revealed a large number who reclined when doing their most active cerebration; and adding and memory tasks were performed slightly better in the prone than in the sitting position (Jones, **18**).[10] There are, no doubt, great individual differences in tension; some persons require more, and others less, for normal adjustment. Whether these differences are constitutional we do not know. Experiments just completed in the acquisition of skill suggests that habituation will produce some reduction in remote tension. If one can perform as adequately with less muscular reinforcement, his 'efficiency ratio' (output over imput) is certainly better than that of his more tense brother.

It would be a mistake, of course, to suppose that the amount of muscular tension could be increased indefinitely without having a detrimental effect upon cortical integration. If my own experiments [11] on this subject were not so convincing, one has only to look about him to see that hyper-tension is inhibitory;—the highly excited doctoral candidate, unable to answer the simplest of questions, the inflexible athlete who makes the worst showing on the field. Such observations make it quite probable that the facilitative effect of tension is definitely limited. This might indeed be expected. Just as the higher nervous integrations demand a certain amount of sustaining action for their proper functioning, so an excess in this action, by its very prominence in the neural flux, will disturb the organization of the performance.

VI

The longevity of a working hypothesis of mental activity depends largely upon the specificity of its implications; that is,

[10] These results are in no wise crucial since there was no indication that the subjects were more relaxed in the prone position. If such a position is occasionally found to be superior, the effect might well be due to increased blood-flow, as suggested by Jones, rather than to any reduction in tension.

[11] These experiments will be part of a forthcoming monograph upon the facilitative and inhibitory effects of muscular tension. In a typical experiment subjects worked under incentives to various degrees of 'effort.' 'Supermaximal effort,' produced by re-encouraging and 'dogging' subjects to win a large money prize, resulted not only in tremendous tonus increments, but a loss of accuracy and—in some cases—speed of performance as well.

the more definite its application to the details of behavior, the greater the ease with which its fundamental tenets may be subjected to test. From the standpoint of scientific progress, it is therefore highly desirable that any new proposal should be applied to as many concrete problems as possible. Upon the basis of its relevance to these problems, the theory may be either amplified, recast or discarded altogether from systematic thinking.

Obviously, the limits of this paper permit only the briefest attempt to achieve such an ideal. Certain of the more important problems in mental dynamics which may be profitably surveyed in terms of our hypothesis are:

1. The relativity of cortical action (in consciousness, dream, hypnosis and sleep).
2. The initiation and cessation of waking activity.
3. The degradation of mental activity under habituation.
4. 'Set' and the facilitation of specific mental activities.
5. 'Effort' and the regulation of behavior under obstruction.
6. The stratification of mental processes.

The following discussions of these topics will serve to indicate the general importance of the muscular processes in the problems of total integrated behavior.

1. *The Relativity of Cortical Action.*—If sleep and waking have relative status only, the close parallelism of the amount of muscular tension with such relativity seems especially significant. The most completely non-conscious state that we know anything about is deep anesthesia. Cortical centers are much more silent here than in normal sleep, concerning which there is at least waking remembrance of time passage. The differential relaxation of normal sleep certainly shows a higher degree of physiological reactivity than does the limp state of the person who has fainted or is under the influence of chloroform.

Dream-states are a type of intermediary between sleep and waking which are hard to account for in physiological terms. We know, however, that dreams are elusive and faintly organized (Dunlap, **5**) and that their report is more likely to be

accurate if we make little attempt to rouse ourselves, but lie rather passive. This last fact suggests a special instance of where too much proprioceptive stimulation would disturb the cerebral integration and hence erase the trace of organization. Reproduction is thus bettered by keeping the summation of intra-organic excitation near the degree present during the formation of the dream.

The resemblance of hypnosis to normal sleep turns out to be physiologically superficial when the criterion of relaxation is applied. Using such a criterion, Bass (**2**) has recently found that the trance approximates the waking much more than the sleeping state. The amplitude of recorded knee-jerks dropped off progressively in the sleeping state, but remained at about the same level in both trance and waking. The fact that one common method of producing hypnosis is to 'talk sleep' may be simply the appeal to a habit system which favors the neglect of certain forms of external stimulation. Upon such a background 'rapport' could be more easily established. In only one instance would it seem doubtful if the hypnotic subject is *less* relaxed than in the normal state; but such 'catalepsy' is likely to be found to involve the differential relaxation of the musculature, with *hyper-tension* in certain parts.[12] The characteristic of the trance thus becomes a peculiar type of cerebral organization rather than an intermediary between sleep and waking.

2. *The Initiation and Cessation of Waking Activity.*—Information on the phenomena which accompany natural waking is very meager. Especially significant from our point of view are the observations of Kleitman (**19**) on puppies. He found a most prominent feature to be a stiffening of the hitherto relaxed abdominal musculature, followed by stretching. Casual observation of the 'rousing up' of humans suggest many instances of lowered tone rapidly restored by similar means. I have noticed, for example, how often the alarm clock drops through my fingers if it is grasped immediately

[12] Incidentally, may not the phenomenon of 'pre-dormant start' be similarly explained in terms of differential relaxation? We recall that Rosenbach (*Zsch. f klin. Med.*, 1879, 1, 358) found a temporary stage of increased irritability just before the subject went to sleep.

upon my first 'startle' from sleep. A bit of stretching not only produces the degree of postural tone necessary for the execution of the grasping act but enables me to *appreciate* its purposiveness as well.

It is fairly obvious how stretching and other activities incidental to waking may serve to facilitate cerebration by restoring a degree of afferent stimulation comparable to that present upon retiring. But it does not necessarily follow that these activities are essential to waking itself. Our existent knowledge is not adequate to this problem. We know, however, that the arousal of centers silent during sleep will depend upon the intensity of stimuli acting to excite them; and that environmental stimuli, while not actually arousing the sleeper, may have an effect upon physiological activity. Among the studies which claim to have demonstrated the excitatory effects of noise during sleep, is that of Laird (**21**) which reports an increase in muscular tension. Although there is no satisfactory technique which will record tonus during sleep, the report is suggestive. Furthermore, motility records show that the body is not uniformly relaxed during the period of sleeping. It is more physiologically active at some periods than at others (Johnson, **17**). Is it not therefore probable that environmental stimuli which impinge upon the organism during periods of heightened motility are the most likely to awaken the sleeper (central summation)?

This brings us to the reverse question of how sleep is produced. Reviewing the doctrine relevant to the topic, Kleitman (**20**) has recently formulated the following theory: "(1) Sleep is an easily reversible inactivity of the highest functional centers. (2) The inactivity is due to a functional break between the cerebral cortex and other parts of the nervous system. (3) The functional break results from a marked decrease in the number of afferent impulses to the sensorium, especially *proprioceptive impulses which depend upon the degree of muscle tonus maintained.* (4) Sleep results most naturally from the fatigue of the neuro-muscular mechanism concerned in the maintenance of muscle tone. (5) In the absence of such fatigue, sleep may result from complete muscular relaxation,

intentional or unintentional. (6) Diurnal alternation of sleep and waking is a 'conditioned' phenomenon."

As I see it, there are but two possible objections to this comprehensive view: (1) Sleep is more probably a relative inactivity of various levels of neural function and (2) the exact mechanism by which tone is originally altered is not apparent. But should the region of the third ventricle turn out to be the sleep center (Hess, **13**), its anatomical connections suggest that it may act upon the postural reflexes, which in turn may reduce cortical irritability after the manner of our analysis. I do not wish to minimize the importance of other sources of cortical stimulation, but my own introspective observations upon retiring seem to indicate that these other sources drop out before proprioceptive stimulation falls below a crucial minimum.

3. *The Degradation of Mental Activity under Habituation.*—The degradation of the conscious aspects of the habituated act have always been an offense to motor theories built around the postulates of the completed reflex arc. I have recently found evidence that tonus decreases with practice in learning nonsense syllables. This throws new light into dark places. Adaptation apparently involves the redintegration or re-systematization of the total action pattern. Not only is it possible for the performance to be carried out with less cortical elaboration, but also these primary centers of control require less supporting (tonic) activity to maintain the level of 'vigilance' requisite to the task. In the development of habits the curve of work-output and the curve of supporting neuromuscular action are at first inversely related. In its initial stages gross tension is necessary to accomplish a minimum of output. Later on a period is reached where a certain minimum of tension accompanies a maximum output. From this point on, theoretically, the two curves should be asymtotic,—subject, of course, to temporary increments acting to compensate for fatigue and other occasional factors which either lower the irritability of the dominant neural integration or favor the dominance of stronger competing tendencies.

4. *'Set' and the Facilitation of Specific Mental Activities.*

—No exposition of the relationship between mental and muscular activity would be complete unless the rôle of specific postural patterns or 'sets' received due attention. As a matter of fact such aspects are quite generally appreciated by existent theory. The chief advantage of Washburn's hypothesis lies, not in its explanation of how the amount of consciousness is determined, but rather of how muscular activity becomes organized into systems and sets which underlie specific performances. We find a similar idea developed by Head in his concept of *postural schemata.* Lashley (**23**) also remarkes that "the evidence for an all-pervading substratum upon which are superimposed reflex and voluntary movement . . . suggests that what I have called the dominant organization (of behavior) may consist of such a postural pattern with adaptive reactions facilitated by it."

That we hear, see, or write better because of some specific muscular preparation or 'set' certainly does suggest the completion of selected reflex curcuits by such means. It is a grave question, however, if every mental process is favored in this special way; that is, has its specific postural substrate. In my own experiments, where simultaneous records were obtained from a number of muscle groups, the pattern of neuromuscular activity during mental arithmetic showed great individual variability. It seems a conservative method, therefore, to treat the known facts of specific facilitation by way of muscular sets as existing upon the background of general facilitation already discussed. This spread of neural excitation, while itself undifferentiated, may produce differential effects by virtue of its muscular *foci.* These foci could be quite definite and hence act selectively in the case of performances involving primarily a muscular response. In the case of more complex performances such as are not directly concerned with overt movement, we should not expect such correspondence.

5. *'Effort' and the Regulation of Behavior under Obstruction.* —Under such categories as distraction, fatigue and effort are usually grouped the conditions which disturb the precision of behavior and the means by which the organism meets these

difficulties. The tendency to compensate for or overcome resistances blocking specifically integrated acts has been noted by many authors.[13] It is as if the organism has standards which it strives to maintain. Such maintenance requires greater or less energy expenditure dependent upon circumstance. Increases in muscular tension apparently act to reinforce reaction systems already in progress in their competition with incompatible tendencies. This has been shown in the work on distraction (Freeman, **7**), sleep losses (Freeman, **9**), and even with long continued exercise of a function [14] (Freeman and Lindley, **10**).

6. *The Stratification of Mental Processes.*—Although the distinction between elementary and complex mental activities has long been made on introspective grounds, differentiation in terms of their bodily concomitants has been generally fruitless. Particularly is this true when we begin to realize that the same anatomical areas of the brain are probably involved in a number of diversified functions. Such activities may, however, be stratified in terms of the amount of neural excitation necessary for the arousal of their hypothetical 'centers.' That is to say, these neural centers may operate on one or many functional levels, dependent upon their thresholds of irritability. The work of Jacobson is most illuminating on this point. The progressive elimination of conscious processes under relaxation instruction suggests that such stratification parallels various degrees of muscular activity. I conjecture that variations in cortical irritability produced after the manner of our analysis account for the correlation found by Jacobson.[15] First to disappear for want

[13] 'Organic regulation' is a basic concept in current biology. And one of the outstanding features in K. Lewin's work is his emphasis upon a similar principle for psychology. (*Cf.* J. F. Brown, The methods of Professor Lewin in the psychology of action and affection, PSYCHOL. REV., 1929, **36**, 200–221.)

[14] *Relative fatigue* of particular neural integrations was found to successively involve greater tonic reinforcement. According to the view developed in that paper it is theoretically possible for the entire musculature to become so exhausted that no further reinforcement can be derived therefrom.

[15] There is some evidence that emotional activities are of lower neurological origin than intellectual reactions. Two explanations are offered for their prepotence in persons rudely awakened, as well as their prominence in exhaustion psychoses when more adaptive behavior has become relatively fatigued: (1) that the excitation-

of a sufficiently lowered threshold are the elaborative 'thought' integrations. Next come the processes of memory and imagination, which require the 'tuning' of cerebral centers to residua of formerly active processes. Finally even environmental stimuli become inadequate for perceptual experience. With sufficient relaxation sleep appears as a natural consequence.

In spite of its lack of objectivity, Jacobson's work is most convincing. Common observation supports his view. We rarely note any tension in connection with perceptual experience. But when we come to effort and fatigue (preceding the break) we attach particular significance to such accompaniments. The 'strained look' is considered one of the external characteristics of thinking. We stand or sit or walk about for shear convenience in perceiving; yet the majority of us find such muscular activity actually helpful in dealing with a knotty problem.

VII

No intention could be less dogmatic than in this attempt to sketch the relationship between mental activity and the muscular processes in terms of a theory of central facilitation and summation. We have seen that the linkage of mental activity with the so-called expressive movements is probably unwarranted and that the endeavor to explain the role of kinæsthesis by reference solely to principles of reflex action is likewise unsound. It is hoped that the evaluation of this latter type of evidence has aroused more questions than it has answered; for should these questions lead to further experimentation, the paper has fulfilled its proper function. Everywhere there are gaps in our knowledge of the relationship as discussed. But the important thing is that these are gaps which may be filled by appropriate research instead of by argument. Such results, when obtained, will prove the ultimate test of our working hypothesis.

Of the many questions to be answered, not a few will involve the repetition of previous research, both with refined

thresholds of suppositious thalamic centers are lower than for cortical centers (hence are prepotent in the absence of normal proprioceptive facilitation) or (2) that *relative fatigue* of the cortical centers would 'release' these lower centers from a normal waking inhibition. It is probable that both factors operate.

techniques and with more subjects. Since the independent investigator is limited to the choice of fine recording with few subjects or gross indication with many, I have made an estimation of the relative merits of several types of measurement now in use (Freeman, **6**). Systems vary from *pressure of a pencil on paper* to *triodic amplification of electrical action currents*. In my opinion neither of these extremes are satisfactory and we have yet to develop a method suitable for use with many subjects.

In view of the probable great individual differences in normal muscular tension, increasing the number of cases is especially necessary. We may find that at any given moment, human reactors may be placed along a graded scale of tonic facilitation; that is, each individual's behavior will be most adequate within certain limits. If his tension rises above or falls below these hypothetical limits, his behavior might conceivably become more or less disintegrated. The specific application of such principles as these to the disorders in personality and the neuroses must await additional work.[16]

We have seen that the majority of experiments have been concerned with the tonus-changes which accompany variations in mental activity. The more suggestive lead now open for exploitation is the observation of the effects of experimentally induced tension on cerebration. Jacobson's work on progressive relaxation necessitates a more objective study and Bills' study is obviously of preliminary character. Projected work of my own aims to clear up some of the issues which have confused this exposition. While they may not accord entirely with the present suggestions, it seems improbable that they will deface the fundamental character of the analysis.

VIII

The working hypothesis herein advanced to explain the facilitative effect of the muscular processes upon mental

[16] In conversation, Professor J. F. Fulton has pointed out certain of these applications, *e.g.*, the peculiar 'mental lassitude' of patients suffering from cord-transection, where cortical stimulation of proprioceptive origin is noticeably reduced. Jacobson has, of course, devoted the greater part of his work to the rôle of tension in the neuroses; but not from this particular point of view.

activity is a part of a larger theory of altered excitability or 'vigilance' by way of humoral and nervous mechanisms. The relationship specifically dealt with may be recapitulated briefly, as follows:

1. Those cortical centers predominantly involved in mental activity have relatively high excitation-thresholds. These thresholds vary with the level of functional activity. Thus, while the same centers might be concerned in both perception and memory, the critical threshold might be lower for the perceptual than for the memorial processes.

2. By appropriate lowering of their thresholds of irritability, cortical centers respond to otherwise inadequate stimuli during waking.

3. Such threshold-alteration is accomplished largely by the spread of excitation irradiating from lower neural centers, particularly those involved in muscular contraction.

4. Cortical action is reinforced and sustained by a continuous stream of proprioceptive impulses. Should such stimulation fall below a certain minimum (as during sleep) cortical neurones would be unable to respond adequately to exteroceptive stimulation of moderate intensity.

5. In the economy of central nervous operation, muscular contraction not only sustains experiential processes in general; the pattern of postural tonus may actually favor the execution of selected acts. It is suggested that many 'sets' are psychological correlates of such qualitative variations.

6. Above a certain maximum muscular contraction may become an inhibitor of precise neural integration. The facilitative effect is only between limits, which vary, presumptively, from individual to individual.

7. The regulation of muscular tension is largely autogenic. Momentary excesses during predicament and distraction are compensatory in function. Habituation reduced the amount of supporting action; relative fatigue increases it.

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THE SIGNIFICANCE OF SPEED IN TEST RESPONSE

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Beginning with the personal equation of the astronomers there has been persistent inquiry concerning the significance of individual differences in quickness of response. In the last decade of the nineteenth century occurred the first definite attempts to relate speed of response to intelligence. During the last 30 years investigations concerned with the significance of speed in test response have revealed three important trends:[1] (1) Several experiments have studied the interdependence of speed in motor performance to determine whether a general motor ability exists. (2) Another series of investigations have compared reaction time and speed of response in other motor performances to intelligence as the tests measure it and as teachers judge it. (3) Employing either scholastic or mental test material a third group of studies have investigated the relationship between rate of performance and ability (power). Certain of the latter have endeavored to discover whether a general speed factor is inherent in mental ability.

The general findings in this rather extensive series of reports may be briefly summarized: (1) The average intercorrelation between performance in motor tests of various kinds[2] is approximately zero and hence performance is not appreciably affected by any general factor. There is, there-

[1] No comprehensive review or listing of the literature will be undertaken here. Bibliographies and summaries which are fairly complete may be found in: P. R. Farnsworth, R. H. Seashore, & M. A. Tinker, Speed in simple and serial action as related to performance in certain 'intelligence' tests, *J. Genet. Psychol.*, 1927, 34, 537–551; R. H. Seashore, Individual differences in motor skills, *J. Gen. Psychol.*, 1930, 3, 38–66; and R. A. McFarland, The rôle of speed in mental ability, *Psychol. Bull.*, 1928, 25, 595–612.

[2] Speed of response in motor tests is discussed here because it is intimately associated with responses on intelligence tests in many experiments.

fore, no general motor ability comparable to 'general intelligence.' Group factors in motor ability evidently exist, however, for scores in similar kinds of performance such as various types of simple motor reaction yield an average intercorrelation of approximately + .60. Apparently there are many motor capacities or abilities, some of which are specific to a considerable degree.[3] (2) Whenever valid measures of intelligence have been employed and when experimental conditions and size of group have been adequate, no appreciable relationship [4] has been discovered between speed of simple reaction or speed of motor response and intelligence. (3) On both intelligence and scholastic tests, speed of response on easy items correlates with level of response (ability, power) by approximately + .40. In both simple and complex mental measurements the average correlation between tests is approximately + .60 when accuracy is held constant experimentally and rate of response is the score. From these latter results it has been inferred that a positive relationship exists between rate of response and ability, and that speed of response is a common factor in mental performance.

The amount of research and discussion devoted to the speed factor in mental and scholastic measurement would seem to indicate that the problem is considered relatively important. Apparently all of the reports in this field have failed to consider certain implications involved in their procedures. The purpose of the present paper is to point out certain neglected aspects of results obtained in these experiments.

In most measurement situations the speed element is present and in many it dominates. This has been true es-

[3] It is understood, of course, that the intercolumnar correlation method and the method of tetrad-differences are employed to demonstrate Spearman's g-factor. Results obtained by the use of these methods have been considered by the writer and, in general, they are in harmony with the conclusions here stated. See H. D. Carter, The organization of mechanical intelligence, *J. Genet. Psychol.*, 1928, 35, 270–285.

[4] In view of the results obtained by Farnsworth, Seashore and Tinker (*op. cit.*) the correlation of about + .60 obtained by McFarland (an experimental study of the relationship between speed and mental ability, *J. Gen. Psychol.*, 1930, 3, 67–97,) between simple reaction and various measures of intelligence cannot be accepted as significant until the results have been confirmed.

pecially in experiments designed to discover (1) relationships between speed of response and intelligence test scores, and (2) the presence of a common speed factor in test performance. The measuring instruments used in these experiments have employed two common methods of scoring: (1) the time-limit method in which the score is number of items completed correctly within a certain number of minutes or seconds; and (2) the work-limit method where the score is number of minutes or seconds required to complete a given task. It is obvious, especially in group testing that the time-limit method has many advantages. Because of this practical consideration, Paterson and Tinker [5] compared the two methods in a speed of reading test in which accuracy of performance was practically constant. It was found that the work-limit method and the time-limit method are equivalent for that test situation, for the coefficient of correlation between the two types of scores, when corrected for attenuation, was + 1.00.

In other speed test situations where accuracy is approximately constant (*i.e.*, easy opposites, completions, arithmetic problems, etc.) or is held constant experimentally [6] one may safely assume that either method of test application will yield scores equivalent to scores obtained by the other method. Also it is probable that the two methods are approximately equivalent in most measurement situations where speed of response is emphasized as it is in most intelligence and scholastic testing.

The common factor in intelligence, as proposed by Spearman,[7] is well known. Evidence for the existence of a general factor in a group of tests involving verbal ability is shown by the intercorrelations. If work-limit scores are practically equivalent to time-limit scores in speed tests it is optional with the investigator whether one method or the other is

[5] D. G. Paterson & M. A. Tinker, Time-limit vs. work-limit methods, *Amer. J. Psychol.*, 1930, 42, 101–104.

[6] See Farnsworth, Seashore and Tinker, *op. cit.*; H. Peak & E. G. Boring, The factor of speed in intelligence, *J. Exper. Psychol.*, 1926, 9, 71–94; and R. A. McFarland, *op. cit.*, 1930.

[7] B. Hart & C. Spearman, General ability, its existence and nature, *Brit. J. Psychol.*, 1912, 5, 51–79; C. Spearman, The theory of two factors, PSYCHOL. REV., 1914, 21, 101–115; and C. Spearman, The abilities of man, New York, Macmillan, 1927.

employed in obtaining scores from which to demonstrate the existence of the general factor. Both Farnsworth, Seashore and Tinker (*op. cit.*) and McFarland (*op. cit.*, 1930) apparently were unaware that time-limit scores and work-limit scores are equivalent under the conditions of their experiments. Inspection of Table 3, p. 540 in the first study and Table 7, p. 90 in the second reveal intercorrelations between work-limit scores on intelligence tests (excluding the reaction time scores) which are approximately what one might expect if the scores were obtained by the time-limit method. In fact we have striking proof of this in the Farnswarth, Seashore and Tinker reference. The average intercorrelation between Army Alpha, Otis Advanced Examination, and the Ohio University Psychological Test for scores in unlimited time when the time-limit method was used is + .643; and when the work-limit method was employed (total times to complete tests) is + .666. Therefore, under the conditions of their experiment they obtained approximately the same average intercorrelations whether the time-limit or the work-limit method was employed. Although their subjects had unlimited time they were instructed to work for speed as well as accuracy. McFarland, employing the work-limit method and keeping accuracy constant experimentally, found average intercorrelations of approximately + .55 between scores on various tasks. He concludes that there is a general factor (speed of response) in the various mental tasks and that the common factor is Spearman's general ability. "Ability in respect to speed, therefore, is an individual trait which is characteristic of mental behavior."

The intercorrelations found by McFarland undoubtedly represent a general speed factor (Spearman's general ability) in the various mental tasks performed by his subjects. The valid application of this generalization to measurements of other and more complex forms of mental behavior awaits the adequate demonstration that time-limit scores are equivalent to work-limit scores in such complex forms of response to mental tasks. Re-analysis of data cited by Farnsworth, Seashore and Tinker, (*op. cit.*) together with the results of

Paterson and Tinker (*op. cit.*) indicate that the two methods of scoring probably are equivalent in the more complex testing situations as well as in tests similar to theirs. Further experimentation, however, is needed to establish this as a fact.

A survey of pertinent investigations reveals results which tend to confirm the contentions of this paper. Whenever speed of response is measured in the same material as ability (power) or when speed is measured in one material and ability in another, both of which are known to involve a common factor (*i.e.*, general intelligence), one finds that speed correlates with ability yielding a coefficient which is representative of the common factor.

Summary and Conclusions

1. A survey of the literature on the relation of speed of response to mental ability shows that no common factor runs through both motor (including reaction time) and mental test responses. However, there are group factors of varying degrees of specificity in motor responses and speed appears to be related to ability in mental and scholastic test responses when speed and ability are measured on the same kind of material.

2. When accuracy of performance is practically constant two equivalent methods of test procedure are available; the time-limit method (amount done in a certain time = achievement, ability or power) and the work-limit method (time to complete a task = speed). Experimental evidence indicates that the two methods are practically equivalent in most mental and scholastic test situations.

3. Wherever time-limit and work-limit methods are practically equivalent, either may be employed to determine the presence or absence of a general factor in test performances. Therefore it is optional whether one employs the term speed or ability to designate the general or group factor.

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PSYCHOLOGICAL JOURNALS: A MINOR CONTRIBUTION TO THE HISTORY OF PSYCHOLOGY

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I have recently had occasion to compile a list of journals directly or indirectly of interest in psychology regardless of where or when published. This list has been extremely illuminating in many respects, especially when considered in connection with the history of experimental psychology.

The most important guide to periodical publications is undoubtedly the *Union List of Serials in Libraries of the United States and Canada* published in 1927, with three supplements in 1930. This list includes approximately 75,000 titles from which I have selected those which seemed to be of greatest importance in psychology. My selections were supplemented by titles from the *Psychological Index* for 1922, and the third volume of *Psychological Abstracts*, 1929. The list upon which this paper is based includes 780 titles, exclusive of the serial bulletins of Universities. Of course the list might have been much longer but in the selection nearly all local medical and educational journals, many general medical, sociological, biological and zoological journals, most general science journals, and the publications of general science societies were excluded. This limitation was justifiable, I believe, because their inclusion would have masked the historical development.

Subject and Geographic Distribution

Table 1 presents data showing the distribution of the 780 titles by subject and country. The subject divisions are empirical rather than logical. In each case but two the group name is descriptive. The exceptions are 'Sense organs' which includes ophthalmology and otology, and 'Psychiatry' which includes nervous and mental diseases and mental hygiene.

The column headed 'miscellaneous countries' includes China, Australia, and South American countries as well as the newer countries of central Europe.

It would seem a safe *a priori* assumption that the number of journals published in a given country is closely related to the number of men working in that country. To test this assumption the rank correlation between the number of extinct and current journals published in, and an estimate of the number of men who have worked or are working in thirteen of the major countries was determined. The number of men who had been engaged in the various countries was estimated from Boring's *History of experimental psychology*. From the total of 896 names in the index 265 were omitted because they belonged to periods earlier than 1800 or because they were engaged in activities other than psychology or its cognate subjects. Each of the remaining names was allocated to its respective country as shown in the last row of Table 1. Of

TABLE 1

SUBJECT AND COUNTRY DISTRIBUTION

Subjects	Countries													
	U. S.	Ger.	France	Eng.	Italy	Russia	Scandinavia	Austria	Holland	Spain	Switzerland	Belgium	Japan	Miscellaneous
Psychology, General	26	46.8	10.5	5.5	4.0	5.5	3.8	1	1	1	3	1	1	7.0
Psychology, Applied	9	12.0	3.0	3.0	1.0	2.0				1				1.0
Psychiatry	46	46.0	16.0	9.0	26.5	18.0	1.0	2	2	3	2	4	1	5.0
Psychoanalysis	3	5.0	1.0	1.0	1.5			5						
Education	27	37.3	4.5	2.0	2.0	3.0	3.8		1	1				15.0
Physiology	6	16.5	10.0	4.0	2.0	2.0			1					1.0
Neurology	9	2.0	3.0	2.0	4.0					1			1	2.5
Philosophy	8	24.3	9.0	1.5	3.0	0.5	.3			1				2.0
Child Study	14	11.0	5.0	6.0	4.0			2	1					3.0
Biology-Zoology	9	5.0	4.0	1.0	4.0	2.0	1.0							
Genetics-Eugenics	6	1.0		2.0										
Sense Organs	10	8.0	6.0	4.0	2.0	1.0	2.0						1	
Anthropology	2	1.0	2.0	3.0	1.0			1	1		1			1.0
Psychical research	2	4.0	6.0	4.0										.5
Phrenology-Magnetism	3	1.0	8.0	3.0	1.0									
General Science	6	2.0	3.0	2.0	1.0			1	1					1.0
Miscellaneous	16	10.0	6.0	5.0			1.0	1	1					2.0
Totals	202	233.0	97.0	58.0	57.0	34.0	13.0	13	9	8	6	5	4	41.0
Men (Boring)	204	237	53	89	12	5	3	8	5	1	4	3	2	265

course this criterion assumes that the number of men important enough to be included in a history is a rather accurate indication of the total number of workers. The correlation, $\rho = .862$, between the ranks of countries represented in the *Psychological Register* and in Boring's *History* to some extent corroborates this assumption. Between the two sets of data discussed above ρ was found equal to .894. Thus the earlier hypothesis that publication is closely related to the number of men engaged is corroborated.

Chronologic and Geographic Distribution

Table 2 presents the distribution of titles by decades for each of the major countries or groups of countries. Each

TABLE 2

Chronologic Distribution

Decades	Countries										
	U. S.	Ger.	England	France	Italy	Other Europe	Asia	Australia	So. Amer.	Canada	Africa
1770–79		2									
1780–89		4									
1790–99		4									
1800–09		4									
1810–19		4	1	1							
1820–29	1	3	1	3		1					
1830–39	2	4	3	6		1					
1840–49	6	5	6	8							
1850–59	4	13	6	8	1	1	1				
1860–69	8	22	5	14	3	4	1				
1870–79	13	26	11	18	7	7	1				
1880–89	24	36	14	27	17	19	1		2		
1890–99	46	71	15	47	24	27	2	1		1	
1900–09	64	104	24	61	30	29	5		6	1	1
1910–19	110	140	31	55	29	37	4	1	10	3	1
1920–29	149	135	43	52	29	42	8	4	8	5	1

number represents the number of periodicals appearing during at least a part of that decade. Germany leads with a journal as early as the 1780's (see infra) and increasing publication since that date. The United States began publishing only in 1820, while in the decade 1920–1929 it had 149 periodicals to Germany's 135. However, this apparent superiority may be

a function of the source of material, inasmuch as more or less local journals published in the United States would be included while similar journals of foreign nations might be omitted.

Chronological and Subject Distribution

A table showing the distribution of subjects by decades has not been prepared although it would be valuable. In place of such a table I have mentioned briefly some of the pioneer journals, in general and abnormal psychology of various countries.

I. Psychology

As shown in Table 2 Germany published the earliest periodical of interest included in my list. This journal surprisingly enough unites the two disciplines that are today considered closest by most psychologists. Its title was *Repertorium der Psychologie und Physiologie* of which two numbers appeared between 1786 and 1788. Following this was the *Psychologisches Magazin*, three volumes of which were published between 1796 and 1798. Some fifty years after the death of this second pioneer journal we find that Germany established a third, the *Archiv für die pragmatische Psychologie* with three volumes between 1851 and 1853. During this fifty years' interval apparently the only other journal containing 'Psychology' in its title was published in Denmark. This was probably general in nature, being entitled *Archiv for psychologie, historie, literatur og kunst.* There were thirteen volumes published between 1824 and 1830.

In England one volume of the *Proceedings* of the Psychological Society of Great Britain appeared for the years 1875–1879, while *Mind* started in 1876. During the decade of the 1880's journals appeared in many countries.

Wundt's *Philosophisches Studien* began in 1881 and existed until 1903 after which it was continued in the *Archiv für die gesamte Psychologie.* In France the Société de psychologie physiologique began its *Bulletin* in 1885 and published seven volumes to 1891. The *American Journal of Psychology* was founded in 1887. Russia began the *Voprosy filosofii i psik-*

hologii in 1889 and it continued until 1917. In this decade the *Buen deseo: periódico mensual de estudios psicológicos* was published (1884–1886) at Matanzas, Cuba.

During the decades since the 1880's many of the journals now in existence have been founded, although there were many which continued only a few years. Examples from both of these groups are to be found in many of the smaller countries of Europe, in China, Japan and India, and in Australia.

II. Mental and Nervous Diseases

In the field of abnormal psychology we again find Germany earliest in publishing. From 1805 to 1806 three numbers of the first volume of the *Magazin für die psychische Heilkunde* appeared. Germany also has the longest lived journal of any to be considered. The *Allgemeine Zeitschrift für Psychiatrie und psychischgerichtliche Medicin* began in 1844 and is still being published.

The *American Journal of Psychiatry* almost rivals this record inasmuch as its precursor, the *American Journal of Insanity*, began in 1844 also. As, however, the name and volume numbering were changed in 1921 it must be considered a broken series. This was also the earliest journal on mental abnormalities to be published in America.

England published the *Journal of Psychological Medicine and Mental Pathology* between 1848 and 1882, and founded the *Journal of Mental Science* in 1853. This latter is still appearing.

In France the *Journal de médecine mentale* with ten volumes between 1861 and 1870 is the pioneer. Italy started the publication of the *Archivio italiano per la malattie nervose* in 1864 and it continued for 28 volumes to 1891. The next decade saw the appearance of the *Psychiatrisches Centralblatt* in Austria (8 volumes, 1871–1878), and the *Bulletin* of the Société de médecine mentale de Belgique in 1873. This last is still being published.

During the 1880's we find two journals beginning in Russia, the *Viestnik klinicheskoi i sudebnoi psikhiatrii i nevropatologii* (13 volumes, 1883–1899) and the *Arkhiv psikhiatrii*,

nevrologii i sudebnoi psikhopatologii (32 volumes, 1883–1899). In Holland the *Psychiatrische en neurologische Bladen* began in 1883. The shortest lived of the journals starting in this decade was the *Asylum Journal* which appeared at Berbice, British Guiana in five volumes between 1881 and 1885.

Since the 1880's psychiatric journals have appeared in many of the smaller countries of Europe, several South American countries, Japan and China. In the first decade of the twentieth century two movements were started that have their own literature.

Psychoanalytic journals apparently began in Germany with the *Jahrbuch der Psychoanalyse*, of which six volumes were published between 1909 and 1914. Of the seven current journals four were started before 1915, three in Germany and Austria and one in the United States. The English and Italian journals started in 1920 while the newest, in France, began in 1927.

In the same year that the first psychoanalytic journal was founded the Connecticut Society for Mental Hygiene began its *Publications* series as the earliest serial publication of the mental hygiene movement. This was followed the next year, 1910, by the *Publications* of the National Committee for Mental Hygiene and in 1917 by *Mental Hygiene*. Foreign publications in this field are conspicuous by their rarity. England, France and Germany are apparently the only countries that have started journals; these began in 1920, 1925 and 1928 respectively.

SUMMARY

An analysis of the geographic, chronologic and subject distribution of 780 journal series in psychology and cognate subjects indicates quite clearly that the volume of publication very closely parallels the extent of research in psychology. A rank correlation of .89 between the number of journals in each of 13 major countries and the number of men from these countries cited in Boring's *History* expresses this relation quantitatively.

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Subscription $6.50. 624 pages annually. Edited by M. F. Washburn, K. M. Dallenbach, Madison Bentley, and E. G. Boring.
Quarterly. General and experimental psychology. Founded 1887.

Journal of Genetic Psychology—Worcester, Mass.; Clark University Press.
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Quarterly. Child behavior, animal behavior, comparative psychology. Founded 1891.

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Bi-monthly. General psychology. Founded 1894.

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Without fixed dates, each issue one or more researches. Founded 1895.

Psychological Index—Princeton, N. J.; Psychological Review Company.
Subscription $4.00. 300–400 pages. Edited by Walter S. Hunter.
An annual bibliography of psychological literature. Founded 1895.

Psychological Bulletin—Princeton, N. J.; Psychological Review Company.
Subscription $6.00. 720 pages annually. Edited by Edward S. Robinson.
Monthly (10 numbers). Psychological literature. Founded 1904.

Archives of Psychology—New York, N. Y.; Columbia University.
Subscription $6.00. 500 pages per volume. Edited by R. S. Woodworth.
Without fixed dates, each number a single experimental study. Founded 1906.

Journal of Abnormal and Social Psychology—Eno Hall, Princeton, N. J.; American Psychological Association.
Subscription $5.00. 448 pages annually. Edited by Henry T. Moore.
Quarterly. Abnormal and social. Founded 1906.

Psychological Clinic—Philadelphia, Pa.; Psychological Clinic Press.
Subscription $3.00. 288 pages. Edited by Lightner Witmer.
Without fixed dates (9 numbers). Orthogenics, psychology, hygiene. Founded 1907.

Psychoanalytic Review—Washington, D. C.; 3617 10th St., N. W.
Subscription $6.00. 500 pages annually. Edited by W. A. White and S. E. Jelliffe.
Quarterly. Psychoanalysis. Founded 1913.

Journal of Experimental Psychology—Princeton, N. J.; Psychological Review Company.
Subscription $6.00. 600 pages annually. Edited by Samuel W. Fernberger.
Bi-monthly. Experimental psychology. Founded 1916.

Journal of Applied Psychology—Baltimore, Md.; Williams & Wilkins Company.
Subscription $5.00. 400 pages annually. Edited by James P. Porter.
Bi-monthly. Founded 1917.

Journal of Comparative Psychology—Baltimore, Md.; Williams & Wilkins Company.
Subscription $5.00 per volume of 450 pages. Three volumes every two years. Ed. by Knight Dunlap and Robert M. Yerkes. Founded 1921.

Comparative Psychology Monographs—Baltimore, Md.; The Johns Hopkins Press.
Subscription $5.00. 400 pages per volume. Knight Dunlap, Managing Editor.
Published without fixed dates, each number a single research. Founded 1922.

Genetic Psychology Monographs—Worcester, Mass.; Clark University Press.
Subscription $7.00 per volume of 500–600 pages. Two volumes per year. Edited by Carl Murchison. Monthly. Each number one complete research. Child behavior, animal behavior, and comparative psychology. Founded 1925.

Psychological Abstracts—Eno Hall, Princeton, N. J.; American Psychological Association.
Subscription $6.00. 700 pages annually. Edited by Walter S. Hunter.
Monthly. Abstracts of psychological literature. Founded 1927.

Journal of General Psychology—Worcester, Mass.; Clark University Press.
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